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ANNEX

A

JOURNAL

OF

NATURAL PHILOSOPHY,

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AND

THE ARTS.

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VOL. XXXIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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HK

PREFACE.

THE Authors of Original Papers and Communications in this Volume, are J. A. De Luc, Esq. F. R. S.; Mr. Thomas Reid, Edinburgh; Luke Howard, Esq.; Alfio Ferrara, M. D.; William Hamilton, Esq.; Dr. Bostock; Dr. Marcet, F. R. S.; Mr. J. Singer; Messrs. Kerby and Merrick; Mrs. A. Ibbetson; R. B.; Mr. R. Porret, jun.; W. N.; Dr. Tuthill, F. R. S.; Dr. Pearson, F. R. S. &c.; John Trotter, Esq.; L. O. C.; Mr. F. Accum; Mr. Charles Sylvester; Lord Gray.

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And of British Memoirs abridged or extracted, Mr. J. Allan; John Davy, Esq.; H. B. Way, Esq.; Rt. Hon. Sir Joseph Banks, Bart. P. R. S. &c.; W. T. Brande, Esq.; W. H. Wollaston, M. D.; P. C. Boadie, Esq.; F. R. S.; T. A. Knight, Esq. F. R. S. Pres. Hort. Soc.; Don Joseph

Rodriguez; Dr. Wm. Roxburgh; Sir H. Davy, F. R. S. &c.; Hon. H. Grey Bennet, Esq.; Mr. Tollard, Sen.

The Engravings consist of: 1. Sections of Plants to shew the manner of the Buds in the Stalks, by Mrs. Agnes Ibbetson. 2. The secret and open Nectaries of Flowers, by the same. 3. An Apparatus by Messrs. Kerby and Merrick, for measuring the sonoriferous vibrations of the Gases. 4. A Compensation Pendulum, by R. B. 5. An economical Lamp. 6. New disposition of Musical Keys, by J. Trotter, Esq. 7. The Growth and Increase of Trees, by Mrs. Ibbetson. 8. An Apparatus for manufacturing Prussian blue. 9. Designs to illustrate the Structure of the Roots of Trees, by Mrs. Ibbetson.

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A JOURNAL

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1812.

ARTICLE I.

On the Interior Buds of all Plants. In a Letter from Mrs. Agnes Ibbetson.

To Mr. NICHOLSON,

SIR,

HAVING shown in my last letter how the various parts of a stem finish in a flower, and I think proved, in the most absolute manner, that each part of a flower is formed by an appropriate part of the stem, peculiarly or separately dedicated to its formation, I shall now turn to the middle of a plant, and give as complete a picture as I have been able to discover of that part, with the various changes produced in it by the manner in which the buds run up the interior of the stem, in all plants whatever, that shoot their bud each year from the root; and the stalks of which grow yearly from the ground; whether perennial or biennial, whether dying down or fresh sown. The plants which are the subject of the present letter will embrace an amazing class; for after the strictest search, and most exact dissections, I can discover only five sorts or divisions in nature, comprising many classes and orders, and which, from their consequence

Plants that rise from the earth yearly.

and paucity of numbers, may perhaps well deserve to form the foundation of a natural method, and open to us that which nature herself designed as the commencement of such a plan. I shall first give the five different manners of shooting of the bud, and then enter into farther details concerning this important subject.

The bud shoots in five different modes, forming so many natural divisions of plants.

1st. All those plants which shoot their bud from the nearest line of life, whether in branch or twig: as trees, shrubs, and semishrubs.

2d. All those plants that rise from the earth each year, having a new stem, let their real existence be long or short, and that shoot their bud from the root.

3d. The plants that have no flower stem, but that have in its stead a rallying point, which is immediately discovered by a band or knot; from which the flower buds proceed, and which is found only in grains and grasses.

4th. Those plants which have no regular flower stem, but which are divided from the last by shooting a few partial vessels, with the line of life, just before flowering, enclosing the flower buds: but which are all concealed together within the cuticle of the leaf: as in the palms, arums, and all plants having grass leaves, without bands or bulbs.

5th. All plants that shoot their buds from a bulb.

These five collections of plants are all I can gather from the most exact examination and dissection of British as well as exotic plants; and it appears to me to lay open that view to the discovery of the system of nature I have so long and so ardently sought. But this subject I shall enter into more fully when better prepared to give satisfaction to the public; at present I shall confine myself to the shooting of the bud in the stem of plants.

Manner of shooting the bud in trees, &c.

Of the 1st example, or manner in which buds shoot in trees, shrubs, and semishrubs, I have already given many descriptions: it is as beautiful a process as nature presents: that so soft, so tender a being, should pass through so hard a substance unhurt, that by the moisture of the pith (retained for the purpose) the wood should be separated into collections of vessels, and made to bend both ways, so as to form a covered way for the bud, that it may pass in the midst, unpressed and unconfined, is a conception that the view of the specimen alone could prove the truth of; but it is so easily seen, that it requires only to strip the bark from a branch of any tree, and plenty of buds will be found just shooting from the interior, making their way through the hard substance. It can never be mistaken by a careful observer for that harsh and diminutive piece of wood, which, when the bark is taken off, appears as passing to each leaf; for this is hard, but the buds are always found at the end, very soft and succulent, and covered with albumen.

Shooting of the bud in plants that rise yearly from the earth.

To the 2d example I shall now turn, namely, that in which the buds shoot each year from the root, and the stem is but annual, let the root be older or not. This for a long time puzzled me beyond measure, and few will conceive the labour the discovery has cost me, and the quantity of herbaceous and annual plants I have dissected before I could perceive the whole truth. Perhaps it even exceeds in beauty and contrivance the shooting of the buds in trees. I had long been convinced, that the bud shot from the root, but except in those plants where it runs across the pith, and where I had traced it occasionally up the wood vessels, I could not discover what became of the buds, after they had disappeared at the beginning of the stem, till I found them again in the axilla of the leaves. I shall now take a pentandria digynia plant, and show the whole process of its growth.

Formation of the heracleum spondylium.

I have already said, that the bud is formed in the interior of the root of annual plants, or such as die down to the ground every year; and shall now show how it continues its way in those plants that cross the pith, and then proceed to the buds that do not cross it. The best way of dissecting for both these purposes, is to take a long succession of plants, each a few days or a week older than the preceding. The alteration this little time produces in the interior is amazing. Taking a very young heracleum spondylium; the preparation for forming these immense leaves are all that appears at first in the plant; and this is all confined to the bark only, which it enlarges. The leaves differ in some measure in their manner of forming from the leaves of trees and shrubs, though they are equally woven: no part is more indebted to those occasional hairs (mentioned in a former letter) than

Buds pass through the wood vessels.

the leaves of this plant. As soon as the midrib of the leaf is completed, and the quantity of vessels for weaving the first row of the cross work of the leaf is finished; it is all rolled together into a spike, and this spike is surrounded by immense hairs, as shown at AA, fig. I, Pl. I. (B B represents one of the hairs much magnified). These soon draw plenty of moisture to mix with the juices to form the pabulum of the leaf. The hairs then disappear, the part unrolls, and the leaf begins to weave itself, as at C D, fig. 2. All this time the root is plain and simple, and the pith of the stem (though frequently crossed by the line of life) showing nothing beside, but its own original figure; and though but a small part of the stem is yet formed, it is employed with all its forces, and juices, in rolling and unrolling the leaf. But no sooner is the weaving of the leaf finished, than the shooting of the bud in the root begins; the knots are soon formed on the line of life, within the centre of the root; the ends break, and two buds shoot from each knot; they pass through part of the wood in the root, and then disappear; for each row of bud has its appropriate wood vessels, up which it then passes. It should seem, that all the pentandria digynia plants appear to have too much flower for the wood vessels to contain, nature therefore has recourse to an expedient of a very curious kind: the pith is divided into compartments by the line of life; and at each compartment the buds are pressed out of the vessel, and ranged across the plant. All the pentandria digynia tribe are umbelliferous, and shoot at different times (but at very short intervals) small collections of flowers. It will be seen therefore, that nature already prepares them for the purpose; dividing them in the stalk as they are to shoot. With what art, what exquisite beauty, nature has managed to keep the pith still in the middle of the stem, in order to retain that moisture necessary to the shooting such a quantity of buds; and yet contrived to secure plenty of room for those buds to spread, and come to perfection: dividing the stalk in umbels as they are afterward to shoot into flowers! See fig. 2, E, F, G, the different divisions; H the line of life: I the pith. When the buds were in the root, they were scattered in a careless manner, and moving in the same direction as they do in the wood of trees, that is horizontally. The wood therefore made way for them. But no sooner do they begin to run up the stem, than the wood-vessels instead of forming a covered way for them, opens and receives them within their apertures. A picture of one of the vessels extremely magnified will give an idea of their formation, see fig. 3: K K are the vessels through which passes the sap; L the interior vessels containing the buds; M the albumen, which always appears above and round the buds. All the buds are tied together as in seeds, see fig. 4. The sap vessels, though forming only a part of the new cylinder, are much larger than in trees. When the buds rise to the top of the stem or axilla of the leaf, where they are to flower, several of the squares join together, and form one sort of flower bud; which afterward divides into umbels.

Buds distinguished from the wood of the leaves.

All buds shooting from the root do not cross the stem in this manner.

But all the plants that shoot their buds from the root, and have flower stems, have not their buds crossing the stem in this manner. In the greatest number of herbaceous plants, the buds runs up the interior of the wood vessels to the place where they are to show themselves; that is, to the top of the plant, or the axilla of the leaf: and are not seen till they get there. Yet take a pretty thick cutting of any of these plants, and keep it a few hours, and you will see the buds growing out of the vessels exactly as the buds of silver or lead grow in the arbor dianæ, or saturni under your eye. It was by such a piece of the plant that I discovered how the buds crept up the interior of the wood vessel, and remained so long concealed.

Bud, how formed in grain or grasses.

I shall now turn to the third division, which embraces grain and grasses of every kind, that possess the peculiar band or knot, which is perfectly unlike every other contrivance in the vegetable world. It exceeds indeed any yet shown; but I fear that at every new proof of this exquisite performance, one must have neither soul nor feeling not to become an enthusiast on the subject, when contemplating such wonders; beholding such astonishing productions. As there is no stalk properly so called, the different parts of the plant are collected in the flat leaf of the grass. Thus the bark, the inner bark, the wood, and the line of life, are all possessed of stripes in the leaf. How then can the two sorts of buds be protruded? by all the different parts meeting in a band, when a collection of each matter is selected to produce the circle of the leaf buds, and form a new leaf. Few operations can be more plain and easily understood than this part of the process; it is attended with an odd kind of contrivance, which shows that nature often makes use of the same means we do to effect the same purpose. When the part is to be selected to form the leaf bud, the rest is tied with a knot, lest it should tear down or unravel (see *o o* fig. 5). Then the vessel selected rises, and soon produces a bud, and when the whole row is completed they join together and form a new leaf under the other: this is repeated three times, but at the fourth knot, when the leaf is produced, it is formed round instead of flat, and a quantity of albumen is generated by the stopping of the sap. The line of life then strikes out of the edge of the leaf, and forms a broad circle in the interior of the band, which is always a forerunner of the bud; immediately knots appear on the line, they break, and the flower buds are seen shooting from the ends; their numbers soon fill the round leaf: the buds are all tied together by the line of life as in seeds, and remain in their enclosure till they are perfectly ready to shoot out at the top of the plant in a spike of grassy flowers. Thus this band or rallying point not only serves to strenghthen the plant and support it, but gives a new way of forming the leaf buds, and of protruding the flower buds; and this is no work of imagination, as I shall now show. Fig. 5 is the part selected to form a new leaf bud. Fig. 6 is the first shooting of the flower bud; and though there is some little part of the mechanism I do not quite understand, still as far as I have described, what with watching and dissection, I am pretty certain of being right, and not misleading those who will venture to follow me. That the flower bud is merely the embryo of the plant, enclosed by a few seminal leaves, and is not covered by the meal till the flower rises as high as P, is a certain truth, since I have dissected them both before and after. That the flour of corn, or meal, is formed of the inner bark juices alone, I have the most positive proof; since it is only in the inner bark vessels it is to be found, even from the root: at each new band it grows more

Formation of the bud in grain.

Use of the knot in grain and grass:

more like wheat, and when the fourth knot is perfected the meal is quite milky and sweet to the taste; and when the embryo is ready to be covered by it, as at P, it is the vessels P P that convey the meal to the embryo, when it is little more than very thick milk, which soon however hardens when spread on the embryo, and when the buds next appear they are covered by it, though before absolutely destitute of it. As in every other flower, each part is produced by its own appropriate matter: the male by the wood, the female by the line of life; the bark produces the scales, and the inner bark the meal. Thus all concur with other plants to show the truth of that fact. All grain as well as grasses are alike in their formation, they differ only in the quantity of meal with which their seeds are covered: there is however in the grasses some little difference in the mechanism, but not worth mentioning.

conveying the meal.

A little difference of mechanism in the grasses.

Though a digression I cannot help here giving a piece of information, which appears to me of no little consequence. We suppose that seed to be the finest for producing wheat-flour, which has on it the greatest quantity of meal. I have repeatedly tried the experiment, and two gentlemen have also essayed the same; to sow a part of a field with refuse wheat, provided the seed is perfect. The difference of product between this and the finest and largest seed that could be procured was not to be discovered. Provided the embryo is strong, the quantity of meal on it signifies little; for the best covered is certainly not the strongest producer. In dissecting wheat I have always found that seed with the largest star or hilum gave the greatest returns, and not the one most covered with meal. Much care should be taken to choose seed from a field where there has been no smut, no corn cryptogamia, and to prefer seed not taken from a thrashing machine, or lime and sand floor, for they all in some measure injure; the first two indeed to a great degree: but its being only thin of flour is far from being against it, on the contrary, the embryo is often the stouter for it. Nature keeping the embryo such a time without meal is surely a hint to us, and shows that it does not strengthen the plant.

Refuse wheat for sowing

would save us much flour.

I now turn to my fourth example evinced in the palms,

4th example.

arums, callæ, and dracontia. These have been arranged with grain and grasses; but they are extremely different in their formation. Though they have (like the grasses) no apparent stem; yet they send their buds from the root just before flowering; by enclosing them in wood vessels, the buds being tied together by the line of life; and this slender piece running up within the cuticle of the leaves unperceived till it strikes out of the axilla, or bosom of the leaf, in a bunch of flowers. But it is very easily detected by dissection, if sought; I have repeatedly taken it out of its place of concealment after watching its progress from the root. So careful has nature been to separate this collection from the leaf; that there is a double cuticle on the way up for the purpose; and I suppose to prevent the contamination of the juices.

The buds mounting in bulbous plants.

In the 5th example, that is in all bulbous roots, the flowers are known to be formed in the root, and to rise by degrees improving till they show themselves perfect flowers at the top of the plant, merely by the lengthening of the peduncle: but no person ever suspected, that this was a repetition of the case of all those plants which rise yearly from the earth; that they all equally draw their buds from the root, and pass it to the top, in the interior of one or many wood vessels, according to the sort or size of the flower. In the ranunculus tribe the flower is so complete, (even in the root) that I have taken it out, and dissected it, and proved that the seeds are already formed in it. The various parts of the flower of a bulb are all formed; it is only the proportions that are not observed in its earlier state. It is a curious truth, that certain parts, as the stamen, germe, and nectaries, are much larger when first forming, than they are when more thoroughly perfected. As to the offsets of a bulb they are merely a second sort of bud; with all the attendant parts the same; or another sort of seed; for the difference between these parts is very trifling, all equally proceeding from a knot in the line of life.

Decreasing of the stems in breadth.

I have thus given an account, as I promised, of the five divisions, which might well form the foundation of a natural method: as a more important point in physiology than the budding of plants could not well be found, or fixed on for the

the purpose. I shall now take notice of an observation, that was one of the first I made when examining herbaceous plants, and dissecting them; "The increase in breadth of "the stems of those plants at a certain time of their growth." It is now accounted for by the running up of the buds, as this must of course enlarge them: it however puzzled me not a little to comprehend, why all plants, that rose yearly from the earth, should decrease so much when coming out into flower; especially as they must at that time require more rather than less sap. But the discovery of the buds solves the whole mystery: the flower being within the plant takes nearly the same quantity for its support, and the increase of the stem falls wholly on the interior buds, which most naturally accounts for it. I shall now close this long letter with a few words on dissections in general.

On dissections in general.

Will it not appear by the specimen of the heracleum spondylium, fig. 2, how very difficult it is to dissect plants, and how necessary it is to take them so as to become acquainted with their continual variations, which are still greater within (if possible) than at the exterior? how impossible that a person, who only dissects a plant a few times, should understand it? I may say without the smallest exaggeration, that several persons might give a picture of the dissection of the same part of a plant; and each might accuse his neighbour of giving a false delineation, and yet each drawing might be just and true; but taken at various seasons, or different ages of the plant. Mirbel has given many excellent specimens much magnified; but in some not keeping to the right cylinder his pipes do not join, and appear therefore placed for nothing. Nature acts not in this manner: all is consistent and useful; and that use most apparent and easy to be understood, if the plant is properly cut. To dissect a plant rightly, so as to produce specimens that will truly explain the nature and habit of a plant, is no easy thing; for it requires the most perfect knowledge of its different parts; which is only to be acquired by long study and constant dissection. The 1st point requisite is to be attentive to keep to the right cylinder, particularly if you mean to halve it; as at fig. 2, for if you do not divide it properly, you will have the back of the bud in one square; the front in another; and no bud in a third. In short, instead of that regularity of figure with the line of life leading up to each bud as in fig. 2, all will be confusion and disorder. I never divide a plant without first marking it with an instrument. I use almost as many different sorts as a surgeon: to separate the cylinders requires a very sharp and cutting instrument, and I am at last driven to the necessity of inventing and contriving my own.

On the instruments used for the purpose.

I noticed above the having frequently found instances, where nature makes use of the same means we should ourselves have had recourse to in the same predicament: and some have expressed surprise at it. But why should we be astonished? whence proceed the ideas of man, but from the suggestions of that Creator whose works we are studying? Is it strange then, that we should find them alike; when proceeding from the same source? The works of nature are certainly infinitely more perfect; and if we studied them with stricter attention, ours would borrow more of that beautiful simplicity, which so admirably distinguishes them. Still both proceed from God alone; though our notions and ideas are so contaminated with the feebleness of man's nature.

I am, Sir,
Your obliged servant,
AGNES IBBETSON.

Fig. 6, the first passing up of the flower buds, or rather the embryo, as Q, to form the flower at PP.

Fig. 7 a horizontal section of the wheat in the middle of the fourth knot or band; where the embryo is generated on the line of life, throwing out two buds at each end; see RR. SS are the bark vessels which form the meal.

II.

An Account of some Experiments on the Combinations of different Metals and Chlorine, &c. By JOHN DAVY, *Esq. Communicated by Sir* HUMPHRY DAVY, *Knt., LL. D.; Sec. R. S*[*].

Introduction.

Combinations of oximuriatic acid similar to oxides.

MY brother, Sir Humphry Davy, appears to me to have demonstrated, in his last Bakerian Lecture, the existence of a class a class of bodies similar to metallic oxides, and consisting of metals in union with chlorine or oximuriatic acid.

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[*] Philos. Trans. for 1812, p. 169.

These combinations are the principal subject of the following pages. I shall do myself the honour of giving an account of the experiments I have made to ascertain the proportions of their constituent parts, and likewise of describing some that have not yet been noticed.

Doctrine of definite proportions.

I shall have to relate also the attempts I have made to ascertain the proportions of sulphur in several sulphurets, and the experiments I have performed to estimate the quantity of oxigen in some metallic oxides. The general analogy of definite proportions led me to both these undertakings. This analogy, it will be perceived, I have constantly kept in view, and have had recourse to, both for detecting inaccuracies in my own experiments, and in considering the results of the experiments of others.

New nomenclature.

As the nomenclature connected with the old hypothesis respecting oximuriatic acid is inconsistent with the new views of this substance, I shall venture to call the compounds of the metals and chlorine to be treated of, by the names which my brother has proposed for them.

1. *On the Combinations of Chlorine and Copper, &c.*

Oximuriatic gas combines with copper in two proportions.

There are two distinct combinations of chlorine and copper, both of which may be directly made by the combustion of this metal in chlorine gas. When the gas was admitted into an exhausted retort containing copper filings, the filings became ignited, a fixed fusible substance quickly formed, and the interior of the retort soon became lined with a fine yellowish brown sublimate. The former substance evidently contains least chlorine, for when it was heated alone in chlorine gas, it absorbed an additional portion, and was converted into the latter. Hence the fixed compound may, in conformity with the principles of Sir Humphry Davy's nomenclature, be called cuprane, and the yellow sublimate, cupranea.

1st compound: Boyle's resin of copper.

Cuprane may be procured in several other ways. It may be obtained by heating together copper filings and corrosive sublimate; and it was thus first discovered by Boyle, who called it resin of copper, from its similitude to common resin,

sin. Two parts of corrosive sublimate, and one part of copper filings, I have found the best proportions of the materials.

Another mode of obtaining it.

It may be obtained by boiling copper filings in muriatic acid, or by exposing slips of copper partially immersed in this acid to the atmosphere. In the last instance, I have found the changes connected with the formation of cuprane rather complicated; the copper exposed receives oxigen from the atmosphere, and acid from the ascending muriatic acid fumes, and is thus converted into a green insoluble salt, and this, absorbing more muriatic acid, slowly passes into the deliquescent muriate, which flowing into the muriatic acid is changed by the action of the immersed copper into cuprane.

Proust's white muriate of copper.

Mr. Proust, the first modern chemist who examined cuprane, and who is commonly considered as the first discoverer of this compound, found it produced by the action of muriate of tin on muriate of copper; he named it white muriate of copper, and ascertained that a similar substance results from the decomposition of the common deliquescent muriate by heat.

Properties of this compound.

Cuprane, by whatever means prepared, possesses the same properties. It is fusible at a heat just below that of redness; and in a close vessel, or a vessel with a very small orifice, it is not decomposed or sublimed by a strong red heat; but if air, on the contrary, is freely admitted, it is dissipated in dense white fumes. It is insoluble in water. It effervesces in nitric acid. It silently dissolves in muriatic acid, from which it may be separated by the addition of water, which precipitates it unaltered; and it is decomposed by a solution of potash; or by heating it with the fused hydrated alkali: when it affords the orange oxide of copper. Its colour, transparency, and texture appear alone to vary. It is generally opaque, of a dark brown colour, and of a confused hackly texture; but I have obtained it by cooling it slowly after it has been strongly heated, of a light yellow colour, semitransparent, and crystallized, apparently in small plates.

2d compound. How procured.

Cupranea is only very slowly formed by heating cuprane in chlorine gas. The best mode, that I have found, of procuring it, is by slowly evaporating to dryness, at a temperature not much above 400 of Fahrenheit, the deliquescent muriate of copper. Thus made, it has the same appearance and the same properties, as when directly formed. It is of a yellow colour, and pulverulent. Exposed to the atmosphere, it is converted, by the action and absorption of water, into the deliquescent muriate; and its colour, during this alteration, changes from yellow first to white, and lastly to green. It is decomposed by heat; and even in chlorine gas when the experiment is made on a pretty large quantity, part of the chlorine is expelled, and assumes the gaseous state, and cuprane remains.

Component parts of both ascertained.

I have employed the same methods for ascertaining the proportions of the constituent parts of both these combinations. I have separated the copper by iron, and the chlorine by means of nitrate of silver.

1st compound.

A solution of 80 grains of cuprane in nitro-muriatic acid, precipitated by iron, afforded 51·2 grains of copper, well washed, and perfectly dried.

A solution of the same quantity of cuprane in nitric acid, precipitated by nitrate of silver, afforded 117·5 grains of horn silver, dried till it ceased to suffer any loss of weight by exposure to a temperature above 500 Fahrenheit.

Since horn silver contains 24·5 per cent of chlorine[*], 80 grains of cuprane appear to contain 51·2 grains of copper and 28·8 of chlorine. And 100 appear to consist of

Component parts.

36 chlorine
64 copper
———
100

2d compound.

A solution of 40 grains of cupranea in water, acidulated with muriatic acid, precipitated by iron afforded 18·8 grains of copper.

And a solution of 20 grains of cupranea in water, precipitated by nitrate of silver, afforded 43 grains of horn silver.

Component parts of horn silver.

[*] This I have ascertained by synthesis; 12 grains of pure silver dissolved in nitric acid, and precipitated with muriate of ammonia, yielded 15·9 grains of fused horn silver. I do not give the particulars of the experiment, which was very carefully made; because the result very nearly agrees with that of Klaproth, and of other chemists.

Hence 100 of cupranea, omitting the very slight loss, appear to consist of

Component parts.

53 chlorine
47 copper

100

Two other muriates.

The deliquescent muriate, and the native muriate of copper of Peru, belong to a class of compounds apparently distinct from the preceding combinations of copper and chlorine.

The deliquescent:

The deliquescent salt is well understood; and its composition may be inferred, independent of its water, from that of cupranea.

The native muriate is less known, I shall therefore relate the experiments I have made on this interesting mineral.

the native muriate of Peru.

The specimen I have examined is part of a very fine one, presented to Sir Humphry Davy by William Jacob, Esq. M. P., and deposited in the Museum of the Royal Institution. It consists of muriate and carbonate of copper, of red oxide of iron, and of green coloured quartz. The muriate is partly crystallized; the crystals, from the trials I have made of them, appeared to be pure, and they were, on that account, made the subject of my experiments.

Its properties.

The crystallized muriate dissolves entirely and without effervescence, in all the acids in which I have tried it, and the deliquescent muriate of copper is in each instance formed, and a combination of brown oxide of copper with the acid employed.

Heated slowly in a bent luted glass tube, connected with mercury, the native muriate affords water and oxigen gas, and the residue is an agglutinated brownish mass, which dissolves in muriatic acid, and gives a greenish precipitate with potash, and is apparently a mixture of brown oxide of copper and cuprane. When the heat is raised rapidly to redness, the water expelled is impregnated with muriatic acid, and muriate of copper. I have obtained from 25 grains of the mineral, heated to redness till gas ceased to be produced, just two cubic inches of oxigen. This expulsion of oxigen seems to be owing to the action of chlorine on the brown oxide to form cuprane; and there is, I have ascertained, a similar production of oxigen when heat is applied to a mixture of the deliquescent muriate and brown oxide of copper.

A submuriate of copper.

From these results, which perfectly agree with those obtained by eminent chemists on the Continent, who have examined different specimens of this mineral, it appears to be a submuriate of copper, differing in a chemical point of view from the deliquescent salt merely in containing a smaller proportion of acid.

Analysis of it.

The following experiments were made with the design of ascertaining the proportions of its constituent parts.

50 grains of the crystals in powder, boiled in a solution of 50 grains of potash, afforded 36·5 grains of brown oxide of copper heated to dull redness.

And 20 grains dissolved in nitric acid, and precipitated by means of nitrate of silver, afforded 12·9 grains of dry horn silver.

Hence, considering the deficiency of weight as indicating the quantity of combined water, 100 of the native submuriate of copper seem to consist of

Its component parts.

$$\begin{array}{l} 73{\cdot}0 \text{ brown oxide} \\ 16{\cdot}2 \text{ muriatic acid} \\ 10{\cdot}8 \text{ water} \end{array} = \left\{ \begin{array}{l} 15{\cdot}8025 \text{ chlorine} \\ \\ {\cdot}47 \quad \text{hidrogen} \end{array} \right.$$

This analysis, allowance being made for the difference of theory, nearly agrees with that of Klaproth.

Methods of forming a similar compound artificially.

Mr. Proust, I believe, first discovered an artificial compound similar to the native submuriate of copper. He obtained it in the preparation of the nitro-muriate of copper, and also by a partial abstraction of the acid of the deliquescent muriate by means of an alkali. I have found that it may be procured in several other ways. It may be made directly by adding the hydrated blue oxide of copper to a solution of muriate of copper; and it may be very readily and economically prepared, by exposing to the atmosphere slips of copper partially immersed in muriatic acid; and it is also produced by the exposure of cuprane to the atmosphere. Its production in the last instance is accompanied with that of deliquescent muriate; and the formation of both seems to be owing to the absorption of water and oxigen; for cuprane I have found, though apparently not in the least acted on by dry oxigen gas, is quickly changed when moistened with water and confined in a jar of this gas, and there is a rapid absorption of the oxigene*.

The results agree.

I have not examined all the specimens obtained by these different methods minutely, though sufficiently, I conceive, to ascertain their identity, and their similarity to the native compound. The colour of all of them is greenish white, like that of the native, in a finely divided state. When heated, they all afford water, oxigen gas, and a mixture of cuprane and brown oxide of copper.

Analysis.

I have analysed only the submuriate precipitated from a solution of muriate of copper by a weak solution of potash.

Fifty grains of this, well washed and dried, boiled in a solution of potash, afforded 36·3 grains of dried brown oxide of copper.

And 20 grains dissolved in nitric acid, and precipitated by nitrate of silver, afforded 12·75 grains of dried horn silver. These results differ so little from those obtained with the native, as fairly to permit the conclusion, that the composition of the artificial and native submuriate of copper is the same.

2. *On the Combinations of Tin and Chlorine, &c.*

Two compounds of tin with oximuriatic acid.

Tin, like copper, is capable of combining with two different proportions of chlorine. The liquor of Libavius, one of the combinations, is directly formed by the combustion of the metal in chlorine gas; and the other, I find, may be produced by heating together an amalgam of tin and calomel. Thus obtained, it is similar to that which may be procured by evaporating to dryness the muriate containing the gray oxide of tin, and fusing the residue in a close vessel. Both are of a gray colour, and of a resinous lustre and fracture; and both inflame, like tin itself, when heated in chlorine gas, and are converted into the liquor of Libavius by the absorption of a fresh portion of chlorine. Hence, as the liquor of Libavius contains the largest proportion of chlorine, it may be called stannanea, and the other compound stannane.

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* I have been informed, that submuriate of copper is sometimes found in the neighbourhood of volcanoes, particularly in that of Vesuvius. By means of the above facts, it is evident that its production might be accounted for in such situations.

Stannane

1st compound.

Stannane is fusible at a heat below that of dull redness; it bears this temperature, if air be nearly excluded, without undergoing any change; but when subjected to a heat as strong as glass will bear without being fused, it appears to be, from the slight fume produced, partially decomposed.

It affords the liquor of Libavius when heated with corrosive sublimate, nitre, red oxide of mercury, or with the hyperoximuriate of potash. In the last three instances, oxide of tin is also formed; and with the hyperoximuriate, the action is so violent, that inflammation is actually produced.

The liquor of Libavius and aurum musivum are formed when stannane is heated with sulphur.

Stannane, by the action of water, appears to be converted into the insoluble submuriate of tin, and the acidulous muriate.

2d compound, or liquor of Libavius.

The stannanea, or liquor of Libavius that I have examined, was made by heating together an amalgam of tin and corrosive sublimate, in the proportions commonly recommended. I have obtained this compound in another way, by treating the concentrated solution of the peroxide of tin in muriatic acid, with strong sulphuric acid; a gentle heat applied to this mixture contained in a retort, expels the fuming liquor, which may be condensed, as usual, in a cold receiver.

Its action on oil of turpentine.

The only new and remarkable property, which I have observed the liquor of Libavius to possess, is its action on oil of turpentine. I was led to make trial of it from an idea of sir Humphry Davy, that the combinations of the metals and chlorine might be soluble in oils. In the first experiment, when I poured the fuming liquor into the oil, inflammation immediately took place, with violent ebullition and production of dense reddish fumes. I have used other specimens of oil of turpentine, expecting a similar inflammation, but without its occurrence, though there has been in every instance a considerable action. The mixture of the two being made in a retort connected with mercury, no gas was generated, oxide of tin appeared to be formed, and a viscid oil was produced, which, like the fat oils, left a permanent stain on paper, and had little smell or taste, and which, digested with alcohol, imparted something which occasioned a permanent cloudiness on the admixture of water, and an odour to me not unlike that of artificial camphor. The action of the liquor of Libavius on the oil of turpentine is worthy of farther inquiry. The preceding account of it, I am aware, is very incomplete; but I trust it will serve to call the attention of chemists to a subject so curious.

Analysis of the 1st compound.

To discover the proportions of tin, and consequently of chlorine, in stannane and stannanea, I have taken advantage of the superior affinity of zinc for chlorine, by means of which the tin is separated in its metallic state.

69·5 grains of stannane, made by heating in a glass tube with a very small orifice an amalgam of tin with calomel, were, with the exception of two grains of metallic mercury, apparently a mere mechanical mixture, entirely dissolved in dilute muriatic acid. A slip of clean zinc, immersed in this solution decanted from the residual mercury, quickly precipitated the tin in a very beautiful plumose form; and this precipitate collected on a filter, and well washed, and dried, and fused into one globule under a cover of tallow in a small glass tube, weighed 42 grains.

As therefore 67·5 grains of stannane contain 42 grains of tin, 100 appear to consist of

62·22	tin
37·78	chlorine
100·	

Analysis of the 2d.

As stannanea is extremely volatile, it is difficult to weigh it with perfect accuracy. The mode I adopted was to pour it into a bottle half full of water, the weight of which was previously ascertained, and to infer the quantity added by the increase of weight.

81·75 grains of stannanea thus weighed in water, afforded when decomposed by zinc 34 grains of tin*.

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* A little muriatic acid was added before the zinc was introduced, to dissolve the oxide of zinc, which, in other similar experiments, I observed was rapidly formed, and which, from the large quantity of hidrogen evolved, appeared to be owing to the decomposition of water, chiefly in consequence of the galvanic effect of the contact of the two different metals, zinc and tin.

Hence 100 of stannanea appear to be composed of

42·1 tin
57·9 chlorine

100·

I am not acquainted with any analytical method for directly ascertaining the proportion of chlorine in either of the two preceding combinations. Nitrate of silver, when immediately applied, will not answer the purpose, because the oxide of silver is partially reduced by the solution of stannane; and an oxide of tin is thrown down in mixture with the horn silver from the liquor of Libavius.

Submuriate of tin.

Mr. Proust, to whom we are indebted for very excellent investigations of the different combinations of copper and tin, first discovered a submuriate of tin. He found that a solution of potash precipitated from the solution of muriate of tin this compound, and not the pure gray oxide of tin.

Its properties.

I have obtained it by this method, and all its properties, which I have observed, are perfectly agreeable to its supposed composition.

It is decomposed by a red heat. Subjected to distillation in a small bent glass tube connected with mercury, no gas was produced, water containing muriatic acid and muriate of tin was expelled, and a sublimate like stannane was formed, and the fixed residue was gray oxide of tin.

It effervesces violently with nitric acid; and strong sulphuric acid expels from it muriatic acid fumes.

It dissolves without effervescence in the muriatic and acetic, and in the dilute nitric and sulphuric acids; and all these acid solutions, as they give a black precipitate with a solution of corrosive sublimate, appear to contain the tin in the state of gray oxide.

Analysis of it.

The complete analysis of this submuriate of tin is difficult. The oxide it contains cannot be accurately separated by potash, nor can nitrate of silver be employed to ascertain the proportion of muriatic acid.

I have found 50 grains of it, dissolved in muriatic acid, to afford, when precipitated by zinc, 31 grains of metallic tin. Now as this submuriate is similar to the submuriate of copper, the analogy being imperfect only in the latter containing the

C 2

peroxide, and the former the protoxide, it is natural to infer, that the proportion of muriatic acid is similar in both. But the proportion of muriatic acid in the submuriate of copper is apparently half of that which exists in the muriate; hence, supposing the composition of the submuriate of tin to be similar, 100 of it will consist of

70·4	gray oxide
19·0	muriatic acid
10·6	water
100·	

Probability alone can be attached to this estimate. I have not given the calculations by which it was made, as their data are liable to objection.

3. *On the Combinations of Iron and Chlorine.*

Two compounds of iron and oximuriatic acid.

As there are two oxides of iron, so there are also two distinct combinations of this metal and chlorine. One may be directly formed by the combustion of iron wire in chlorine gas; it is that volatile compound described by sir Humphry Davy in his last Bakerian Lecture, which condenses after sublimation in the form of small brilliant iridescent plates.* The other, I find, may be procured by heating to redness, in a glass tube with a very small orifice, the residue which is obtained by evaporating to dryness the green muriate of iron; it is a fixed substance requiring a red heat for its fusion; it is of a grayish but variegated colour, of a metallic splendour, and of a lamellar texture. As it absorbs chlorine when heated in this gas, and becomes entirely converted into the volatile compound; and as the volatile compound may likewise be obtained by heating in a glass tube, nearly closed, the residue from the evaporation of the red muriate, it is evident, that the fixed compound contains less chlorine than the volatile, and that the former, consequently, may be called ferrane and the latter ferranea.

2d compound.

1st compound.

Its solution in water.

Ferrane dissolves in water and forms the green muriate of iron; but the solution of the whole substance is not complete. There is always left a small and variable quantity of black oxide, which may be considered, on account of its variability, in a state of mechanical mixture, rather than of chemical union with the ferrane.

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* Journal, vol. XXIX, p. 226.

Solution of the 2nd. Ferranea is entirely soluble in water. The solution is identical with the red muriate of iron.

Analysis The analysis of both these compounds is easily effected by means of nitrate of silver.

of the 1st compound: 50 grains of ferrane were put into water: the insoluble residue separated from the solution by decantation; washed, dried, and heated to redness for a minute, previously moistened with oil, weighed 3 grains, and was in the state of the black oxide, being attracted by the magnet. The solution entire, precipitated by nitrate of silver, afforded 102·5 grs of dried horn silver, which indicating 25·1125 grs of chlorine, the proportion of iron, omitting the 3 grains of oxide, appears to be 21·8875. And hence 100 of ferrane seem to consist of

53·43 chlorine
46·57 iron

100·

and of the 2d. Ferranea is not easily obtained in considerable quantities, I have been obliged in consequence to operate upon small portions. The subject of analysis was procured by sublimation from the residue by evaporation of the red muriate, 20 grs of this, in brilliant scales, were weighed in water. The solution, precipitated by nitrate of silver, yielded 53 grs of dried horn silver. Hence 100 of ferranea appear to consist of

64·9 chlorine
55·1 iron

100·

(To be concluded in our next.)

III.

III.

METEOROLOGICAL JOURNAL.

1812.	Wind	PRESSURE. Max.	Min.	Med.	TEMPERATURE. Max	Mi.	Med.	Evap	Rain	
7th Mo.										
JULY 1	S W	29·70	29·44	29·570	63	52	57·5	—	·29	☽
2	Var	29·56	29·40	29·480	65	47	56·0	—	·53	
3	N	30·00	29·56	29·780	61	42	51·5	—		
4	S W	30·05	30·01	30·030	61	42	51·5	·45		
5	S W	30·02	29·96	29·990	63	51	57·0	—		
6	N W	30·27	30·02	30·145	67	50	58·5	—		
7	N	30·29	30·27	30·280	72	51	61·5	·43		
8	E	30·33	30·29	30·310	71	46	58.5			●
9	N E	30·33	30·29	30·310	73	50	61·5			
10	N	30·39	30·29	30·340	72	51	61·5			
11	N	30·29	30·16	30·225	69	54	61·5			
12	N W	30·17	30·16	30·165	66	41	53·5			
13	N W	30·19	30·16	30·175	64	52	58·0			
14	Var.	30·19	30·17	30·180	64	46	55·0		·04	
15	Var.	30·17	30·05	30·110	69	50	59·5		·01	
16	E	30·05	29·95	30·000	65	55	60·0	—		☾
17	Var.	30·14	29·95	30·045	67	56	61·5	—		
18	S E	30·10	30·00	30·050	75	56	65·5	·37		
19	S W	30·00	29·74	29·870	73	55	64·0	—	·17	
20	W	29·85	29·70	29·775	75	50	62·5	—	·34	
21	W	29.96	29·94	29·950	65	45	55·0	·35		
22	S W	30·09	29·96	29·025	63	42	52·5	—	·15	
23	S W	30·09	29·94	29·015	65	52	58·5	—		
24	S W	29·94	29·78	29·860	62	58	60·0	—	·26	○
25	S W	29·79	29·78	29·785	71	57	64·0	·55	·03	
26	N W	29·85	29·79	29·820	68	49	58·5	—		
27	Var.	29·66	29·60	29·630	61	48	54·5	—	1·00	
28	S W	29·66	29·65	29·655	64	50	57·0	—		
29	W	29·80	29·66	29.730	63	49	56·0	·55	·22	
		30·39	29·40	29·975	75	41	58·34	2·70	3·04	

The observations in each line of the Table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

NOTES.

NOTES.

Seventh Month. 1. Much wind: very cloudy: rain at intervals through the day and night. 2. Fair a. m. Thunder showers with hail, p. m. 3. Cloudy: a few drops of rain. 4 The wind veered gradually from N. by E. to S.W. 5. Wind moderate. 22. Thunder and hail***.

RESULTS.

Winds variable.

Barometer: highest observation 30·39 inches; lowest 29·40 inches;
Mean of the period 29·975 inches.

Thermometer: highest observation 75°; lowest 41°;
Mean of the period 58·34°.

Evaporation (in 21 days, the rest being lost by an accident) 2·70 inches.

Rain 3·04 inches.

PLAISTOW. L. HOWARD.
Eighth Month, 17, 1812.

IV.

Chemical Researches on the Blood, and some other Animal Fluids. By WILLIAM THOMAS BRANDE, *Esq., F.R.S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society*[*].

I. *Introduction,*

IN the following pages I shall have the honour of laying before this Society an account of some experiments upon the blood, which were originally undertaken with a view to ascertain the nature of its colouring matter. The difficulties attendant on the analysis of animal substances have rendered some Analysis of animal substances difficult.

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[*] Philos. Trans. for 1812, p. 90.

some of the results less decisive than I could have wished, but I trust, that the general conclusions to which they lead will be deemed of sufficient importance, to occupy the time of this body.

Iron in the blood.

The existence of iron in the blood was first noticed by Menghini*, and its peculiar red colour has been more recently attributed to a combination of that metal with phosphoric acid by Messrs. Fourcroy and Vauquelin†. The very slight discoloration occasioned by the addition of infusion of galls to a solution of the colouring matter, under circumstances most favourable to the action of that delicate test of iron, first led me to doubt the inferences of those able chemists; and subsequent experiments upon the combinations, to which they allude, tended to confirm my suspicion, and induced me to give up no inconsiderable portion of the time which has elapsed since the last meeting of this Society, to the present investigation.

Examination of the chyle and lymph.

An examination of the chyle and of lymph, in order to compare their composition with that of the blood, formed an important part of this inquiry; especially as those fluids have not hitherto been submitted to any accurate analysis, on account of the difficulty of procuring them in sufficient quantities, and in a state of purity. While engaged in assisting Mr. Home in his physiological researches, several opportunities occurred of collecting the contents of the thoracic duct under various circumstances, and in different animals; on other occasions Mr. Brodie has kindly furnished me with the materials for experiment.

II. *On the Composition of Chyle.*

Contents of the thoracic duct variable.

The contents of the thoracic duct are subject to much variation. About four hours after an animal has taken food, provided digestion has not been interrupted, the fluid in the duct may be regarded as pure chyle: it is seen entering by the lacteals in considerable abundance, and is of a uniform whiteness throughout. At longer periods after a meal, the quantity of chyle begins to diminish, the appearance of the fluid in the duct is similar to that of milk and water; and lastly, where the animal has fasted for twenty-four hours or longer, the thoracic duct contains a transparent fluid, which is pure lymph.

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* Vincentius Menghinus de Ferrearum Particularum Progressu in Sanguinem. Comment. Acád. Bonon. T. 2, P. 2, page 475.

† Système des Conn. Chym. Vol. 8, p. [Vol. 9, p. 152. C.]

Properties of the chyle.

A. The chyle has the following properties.

1. When collected without any admixture of blood, it is an opaque fluid of a perfectly white colour, without smell, and having a slightly salt taste, accompanied by a degree of sweetness.

2. The colour of litmus is not affected by it, or that of paper stained with turmeric; but it slowly changes the blue colour of infusion of violets to green.

3. Its specific gravity is somewhat greater than that of water, but less than that of the blood; this, however, is probably liable to much variation.

Separates into two parts.

4. In about ten minutes after it is removed from the duct, it assumes the appearance of a stiff jelly, which in the course of twenty-four hours gradually separates into two parts, producing a firm and contracted coagulum, surrounded by a transparent colourless fluid. These spontaneous changes, which I have observed in every instance where the chyle was examined at a proper period after taking food, are very similar to the coagulation of the blood and its subsequent separation into serum and crassamentum; they are also retarded and accelerated by similar means.

Properties of the coagulum.

B. 1. The coagulated portion bears a nearer resemblance to the caseous part of milk than to the fibrine of the blood.

Action of alkalis,

2. It is rapidly dissolved by the caustic and subcarbonated alkalis. With solutions of potash and soda it forms pale brown compounds, from which, when recent, a little ammonia is evolved. In liquid ammonia the solution is of a reddish hue.

acids,

3. The action of the acids upon these different compounds is attended with nearly similar phenomena, a substance being separated intermediate in its properties between fat and albumen. Nitric acid added in excess redissolves this precipitate in the cold, and sulphuric, muriatic, and acetic acids when boiled upon it for a short time.

alcohol and ether,

4. Neither alcohol nor ether exerts any action upon the

coagulum of chyle; but of the precipitate from its alkaline solution they dissolve a small portion, which has the properties of spermaceti: the remainder is coagulated albumen.

and sulphuric acid.

5. Sulphuric acid very readily dissolves this coagulum, even when diluted with its weight of water; and with the assistance of heat, it is soluble in a mixture of one part by weight of acid, with four of water; but when the proportion of water is increased to six parts, the dilute acid exerts no action upon it. I was surprised to find, that the alkalis produced no precipitation in these sulphuric solutions when heat had been applied in their formation, and where a small proportion only of the coagulum had been dissolved; and was therefore led to examine more particularly the changes, which the coagulum had undergone by the action of the acid.

The solution in this acid examined.

On evaporating a solution of one drachm of the coagulum in two ounces of dilute sulphuric acid (consisting of one part by weight of acid with three of water) down to one ounce, a small quantity of carbonaceous matter separated, and the solution had the following properties.

It was transparent, and of a pale brown colour.

Neither the caustic nor carbonated alkalis produced in it any precipitation, when added to exact saturation of the acid, or in excess.

Infusion of galls, and other solutions containing tannin, rendered the acid solution turbid, and produced a more copious precipitation in that which had been neutralized by the addition of alkalis.

When evaporated to dryness, carbonaceous matter was deposited, and sulphurous acid evolved, with the other usual products of these decompositions.

Action of nitric acid on the coagulum.

6. On digesting the coagulum in dilute nitric acid, consisting of one part by weight of the acid to fifteen of water, it was speedily rendered of a deep brown colour, but no other apparent change was produced for some weeks; when, on removing it from the acid at the end of that period, it had acquired the properties of that modification of fat, which is described by Fourcroy under the name of adepocire*.

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* Mém. de l'Acad. des Sciences, 1789.

A mixture of one part of nitric acid with three of water acted more rapidly upon the coagulum of chyle; a portion of it was dissolved, and, when the acid was carefully decanted from the remainder, it was found to possess the properties of gelatine. But when heat was applied, or when a stronger acid was employed, the action became more violent, nitrogen and nitric oxide gas were evolved, and a portion of carbonic acid and of oxalic acid was produced.

Action of muriatic acid,

7. Muriatic acid in its undiluted state does not dissolve the coagulum of chyle; but when mixed with an equal quantity of water, or even more largely diluted, it dissolves it with facility, forming a straw-coloured solution, which is rendered turbid when the alkalis are added to exact saturation, but no precipitate falls, nor can any be collected by filtration. When either acid or alkali is in excess in this solution, it remains transparent.

of acetic,

8. Acetic acid dissolves a small portion of the coagulum of chyle, when boiled upon it for some hours. As the solution cools, it deposits white flakes, which have the properties of coagulated albumen.

oxalic, &c.

9. The action of oxalic acid is nearly similar to that of the acetic, but neither citric, nor tartaric acid, exerts any action upon this coagulum.

Destructive distillation.

10. The destructive distillation of this substance affords water slightly impregnated with carbonate of ammonia, a small quantity of thin fetid oil, and carbonic acid and carburetted hidrogen gas.

Coal.

The coal which remains in the retort is of difficult incineration; it contains a considerable portion of muriate of soda and phosphate of lime, and yields very slight traces of iron.

Properties of the serous part of chyle.

C. 1. The serous part of the chyle becomes slightly turbid when heated, and deposits flakes of albumen.

Crystals in it.

2. If after the separation of this substance the fluid be evaporated to half its original bulk, at a temperature not exceeding 200° Fahrenheit; small crystals separate on cooling, which, as far as I have been able to ascertain, bear a strong resemblance to sugar of milk; they require for solution about four parts of boiling water, and from sixteen to twenty parts of water of the temperature of 60°. They are sparingly soluble in boiling alcohol, but again deposited as

the solution cools. At common temperatures alcohol exerts no action upon them. The taste of their aqueous solution is extremely sweet. By nitric acid they are converted into a white powder of very small solubility, and having the properties of saccholactic acid, as described by Mr. Scheele*.

The form of the crystals I could not accurately ascertain even with the help of considerable magnifiers. In one instance they appeared oblique six-sided prisms, but their terminations were indistinct.

Some of the crystals, heated upon a piece of platina in the flame of a spirit lamp, fused, exhaled an odour similar to that of sugar of milk, and burnt away without leaving the smallest perceptible residuum.

Destructive distillation.

3. The destructive distillation of the serous part of chyle afforded a minute quantity of charcoal, with traces of phosphate of lime, and of muriate of soda and carbonate of soda.

III. *Analysis of Lymph.*

Pure lymph.

The food found in the thoracic duct of animals that have been kept for twenty-four hours without food is perfectly transparent and colourless, and seems to differ in no respect from that which is contained in the lymphatic vessels. It may therefore be regarded as pure lymph.

Its properties.

It has the following properties†.

1. It is miscible in every proportion with water.
2. It produces no change in vegetable colours.
3. It is coagulated neither by heat, nor acids, nor alcohol: but is generally rendered slightly turbid by the last reagent.
4. When evaporated to dryness, the residuum is very small in quantity, and slightly affects the colour of violet paper, changing it to green.
5. By incineration in a platina crucible the residuum is found to contain a minute portion of muriate of soda; but I could not discover in it the slightest indications of iron.

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* Chemical Essays, No. XVII,

† The term lymph has been applied indiscriminately to the tears, to the matter of encysted dropsy, and to some other animal fluids.—Vide Aikin's Dictionary of Chemistry and Mineralogy, Art. Lymph.

6. In

Analysis of it by electricity.

6. In the examination of this fluid, I availed myself with some advantages of those modes of electrochemical analysis, which on a former occasion I have described to this Society[*].

When the lymph was submitted to the electrical action of a battery, consisting of twenty pairs of four inch plates of copper and zinc, there was an evolution of alkaline matter at the negative surface, and portions of coagulated albumen were separated. As far as the small quantities on which I operated enabled me to ascertain, muriatic acid only was evolved at the positive surface.

IV. *Some Remarks on the Analysis of the Serum of Blood.*

Serum of blood.

This fluid has been so frequently and fully examined by chemists, that I shall not enter into a detailed account of its composition, but merely state such circumstances respecting it as relate particularly to the present inquiry, and have not hitherto been noticed by the experimentalists to whom I have alluded.

Serosity.

The fluid which oozes from serum that has been coagulated by heat, and which, by physiologists, is termed *serosity*, is usually regarded as consisting of gelatine, with some uncombined soda, and minute portions of saline substances, such as muriate of soda and of potash, and phosphate of lime and of ammonia. Dr. Bostock regards it as mucus[†].

From some experiments which I made upon the serum of blood, on a former occasion, I was induced to regard the serosity as a compound of albumen with excess of alkali, and to consider the coagulation of the serum analogous to that of the white of egg, and of the other varieties of liquid albumen.

Examination of it.

To ascertain this point, and to discover whether gelatine exists in the serum, I instituted the following experiments.

Two fluid ounces of pure serum were heated in a water bath until perfectly coagulated: the coagulum, cut into pieces, was digested for some hours in four fluid ounces of distilled water, which was afterward separated by means of a filter.

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[*] Phil. Trans 1809, p. 373. [Journ. vol. XXVI, p. 14.]

[†] Transactions of the Medical and Chirurgical Society of London, vol. I, p. 75.

The clear liquor reddened turmeric paper, and afforded a copious precipitation on the addition of the infusion of galls, and when evaporated to half an ounce, it gelatinized on cooling. It was rendered very slightly turbid by the addition of dilute sulphuric and muriatic acid; but alcohol produced no effect.

From the result of these trials, it might have been concluded, that gelatine was taken up by the water, but as an alkaline solution of albumen forms an imperfect jelly when duly concentrated, and as albumen and gelatine are both precipitated by tannin, I was inclined to put little reliance on the appearances just described, until I had examined the solution by the more accurate method of electrical decomposition.

It contained albumen,

Upon placing it in the Voltaic circuit my suspicions were justified, by the rapid coagulation which took place in contact with the negative wire. I therefore made some other experiments in order to corroborate this result.

One fluid ounce of pure serum was dissolved in three of distilled water: the conductors from a battery of thirty pairs of four inch plates were immersed in this solution at a distance of two inches from each other; the electrization was continued during three hours and a half, the solid albumen being occasionally removed; at the end of that period, no farther coagulation took place, and a mere decomposition of the water was going on.

but no gelatine,

Having ascertained in previous researches, that gelatine is not altered during the electrical decomposition of its solution carried on as just described, my object in this experiment was, to ascertain whether any gelatine remained after the complete separation of the albumen had been effected. I accordingly examined the water from which the coagulated albumen had been removed, and found that it was not altered by infusion of galls, nor did it afford any gelatine when evaporated to dryness.

Action of muriatic acid on it.

Two fluid ounces of dilute muriatic acid were added to one of serum. The mixture immediately assumed a gelatinous appearance; it was heated, and a more perfect coagulation of the albumen took place; the liquid part was separated by a filter. No effect was produced upon it by Voltaic electricity, nor did infusion of galls occasion any precipitation.

Farther test of its composition.

I repeated the first experiment with the addition of twenty drops of a solution of isinglass to the serum. The liquid which now separated, after the albumen had been entirely coagulated by the action of electricity, was copiously precipitated by infusion of galls.

It consists of albumen combined with much alkali.

It may be inferred from these experiments, that gelatine does not exist in the serum of the blood, and that the serosity consists of albumen in combination with a large proportion of alkali, which modifies the action of the reagents commonly employed, but which is readily separated by electrical decomposition.

Slight traces of iron in it.

To ascertain whether iron exists in the serum of the blood, one pint was evaporated to dryness in a crucible, and gradually reduced to a coal, which was incinerated and digested in muriatic acid, to which a few drops of nitric acid were added; some particles of charcoal remained undissolved; the solution was saturated with ammonia, which afforded a copious precipitation of phosphate of lime, accompanied with slight traces only of oxide of iron.

V. *Some Experiments upon the Coagulum of Blood.*

Coagulum of blood.

Mr. Hatchett's valuable researches on the chemical constitution of the varieties of coagulated albumen have shown, that this substance varies but little in its properties, whether obtained from the crassamentum of the blood, or from washed muscular fibre, or other sources; but that the proportion of earthy and saline matter is different in the different varieties*.

It will also be remarked, on referring to the dissertation which I have just quoted, that the ashes obtained by incinerating the coal left after the destructive distillation of albumen, did not contain any appreciable proportion of iron.

Inquiry whether the colour of blood be owing to iron.

Assuming the existence of iron in the colouring matter of the blood, I made the following experiments upon the crassamentum of that fluid.

Two pints of blood were collected in separate vessels.

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* Phil. Trans. 1800, p. 384.

The one portion was allowed to coagulate spontaneously; the other was stirred for half an hour with a piece of wood, so as to collect the coagulum, but to diffuse the principal part of the colouring matter through the serum. These two portions of coagulum were now dried in a water-bath, and equal weights of each reduced in a platina crucible to the state of coal, which afterward was incinerated. The ashes were digested in dilute nitromuriatic acid, and the solution saturated with liquid ammonia, in order to precipitate the phosphate of lime, as well as any iron which might have been present.

The precipitates were collected, dried, and treated with dilute acetic acid, by which they were almost entirely dissolved; some very minute traces only of red oxide of iron remaining, the quantity of which was similar in both cases, and so small as nearly to have escaped observation.

It appeared not.

It is reasonable to infer, that; if the colouring matter of the blood were constituted by iron in any state of combination, a larger relative proportion of this metal would have been discoverable in the former than in the latter coagulum; but frequent repetitions of these experiments have shown, that this is not the case, and the following result appears to complete the evidence on this subject.

This farther confirmed.

The colouring matter of a pint of blood was diffused by agitation through the serum, from which it was allowed gradually to subside, the coagulum having been removed: after twenty-four hours, the clear serum was decanted off, and the remainder containing the colouring matter, after having been evaporated to dryness, was incinerated, and the ashes examined as in former experiments. But the traces of iron were here as indistinct as in the other instances above mentioned, although a considerable quantity of the colouring matter had been employed.

The minutiæ of analysis I have purposely excluded, as leading into details which would exceed the proper limits of this paper, and unnecessary in the present investigation; I shall now merely dwell on the principal results which have been obtained, and on the general conclusions which these afford.

(To be concluded in our next.)

V.

V.

On the Nature of falling Stars and the large Meteors, in Answer to Mr. John Farey, *Senior. In a Letter from Mr.* G. J. Singer.

To W. NICHOLSON, Esq.

SIR,

Mr. Farey's hypothesis of falling stars.

THE appearance of a second communication from your correspondent, Mr. Farey, on the nature of falling stars*, leads me to offer a few observations on that subject, which would have followed his first paper, had not the obviously hypothetical nature of his suggestion appeared to render any remark unnecessary.

Mr. Farey notices the electrical nature of these appearances, and their frequent occurrence in "clear frosty nights," and the clear intervals of "showery weather†:" and observing that such "states of the air are best adapted, by its "clearness, for seeing the smaller stars and planets," supposes "that these phenomena are occasioned by an al-"most infinite number of *satellitulæ*, or very small moons, "constantly revolving round the Earth, in all possible di-"rections, and appearing only during the very short time "that they dip into the upper part of the atmosphere each "time they are in *perigee*: and that no step seems wanting "in the degree of this dip into the atmosphere, and their "consequent brightness, length, and slowness of courses, "&c., between the smallest instantaneous *shooting stars*, "and the largest *meteors*, (such as that of August, "1783.")

His assumption of their being explicable without electricity.

This is the substance of Mr. Farey's first communication; and apparently amounts to an *assumption*, that the phenomena may be explained without electricity; *if it be admitted*, that *clear* weather and the *absence* of *twilight*, *moonlight*, &c., are *essential* for their observance: and that planetary bodies may move with *immeasurable velocity*; and appear luminous *only when they dip into our atmosphere*.

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* Journal, vol. XXXII, p. 269.

† Ib. vol. XXX, p. 286.

Shooting stars and large meteors not the same.

If we were to accede to all these *suppositions*, still I presume it would be impossible without manifest absurdity, to refer two appearances so distinct and dissimilar as *instantaneous* shooting stars, and large *progressive* meteors, to the *same cause*.

Facts contradictory to the conditions assumed by Mr. Farey.

But the supposed conditions for the appearancee of luminous meteors are not necessary. Mr. Forster has noticed their occurrence not only in clear weather, but "when cirrocumulus and thunder clouds abound." Mr. Morgan described them darting from the vertex of a bright conical stream of the northern lights; and Beccaria relates minutely the occurrence of a very remarkable one, an hour after sunset.

Numerous observations to the same purpose by the author.

For ten years my attention has been much occupied by these phenomena; I have observed many thousands of them in various situations, and under almost every possible diversity of circumstances. Of the smaller meteors (the shooting stars) I have frequently counted 40 and 50 in an hour, in the brightest *moonlight* nights of summer. I have seen them when no cloud has been apparent, and when the atmosphere has teemed with clouds; and have occasionally observed them when the rays of the Sun had scarcely ceased to illumine the atmosphere.

Meteors seen by him.
1st.

Of the larger progressive meteors I have seen but three; one of these occurred in bright light, at 6 o'clock on a summer's evening. Its motion was apparently rectilinear, and in a horizontal direction from east to west. I had an opportunity of comparing the accounts of several other observers, they nearly coincided with my own observation. The meteor left no visible luminous track, but was followed by a luminous conical tail nearly three times the length of its apparent diameter. It left the field of view to which I was confined, without dispersion. The second meteor I observed at three o'clock in the morning, in the month of April, 1806. It descended in a curve from a considerable height in the north to within an apparently short distance from the Earth, when it dispersed in luminous particles; it left no impression of a luminous track; its form was spherical, and its

2d.

its size apparently equal to the full moon, which it surpassed considerably in brightness. I had two intelligent observers with me at the time, and an excellent opportunity was afforded for this comparison, as the moon was shining, and the atmosphere unobscured by clouds. 3d. The last progressive meteor I have had an opportunity of observing occurred on a cloudy, but moonlight night in august 1808, at one o'clock. It was smaller than either of the preceding, and of a bright red colour; it described a curve of short radius; its course was nearly from north to south, appearing and disappearing nearly at the extremities of the same horizontal line; it dispersed in luminous particles; no track or train of light was observed.

Smaller meteors very different, and apparently electrical.

The peculiarities of smaller meteors are very different. They move with inconceivable velocity; their light is less brilliant; their course usually rectilinear; their appearance frequent, and attendant on states of the atmosphere known to be most connected with its electrical changes. Like lightning they more frequently strike from one part of the atmosphere to another, than from the atmosphere to the Earth; and like it also, when they appear to strike the Earth, they leave no evidence of a moon, or planetary body, having done so.

Difference of large meteors.

Large meteors have rarely dispersed over any spot within reach of observation, but stony bodies have been found; but this has not to my knowledge been ever the case with falling stars, or meteors of a similar nature. From this circumstance, and from the different appearance of their light, the different velocity with which they move, the frequent appearance of the one, and the rare occurrence of the other, I think the two kinds of meteors are distinctly defined, and decisively separated.

Occasional streaks of light in the track of the smaller may be differently accounted for.

With regard to the streaks of light sometimes seen in the track of shooting stars, I am rather inclined to think with Mr. Farey it may be an optical illusion; but I confess this conclusion is in the highest degree doubtful; though its accuracy does not appear to me capable of being easily ascertained, as various equally probable explanations may be given of the phenomenon. No light was apparent in the track of either of the *progressive* meteors I have observed, and the occurrence of such an appearance as an attendant on rapid motion may be consistently accounted for without reference to optical illusion.

Nature of these bodies not yet clear.

In the present state of our knowledge it is certain no positive conclusion relative to the nature of meteors is warrantable; but, in the absence of precise and accurate views, that explanation should be preferred, which is most extensively applicable to the known peculiarities of their appearance.

The large meteors.

The larger progressive meteors are, I think, at present perfectly mysterious. They are certainly not connected with any of the obvious phenomena of electricity; and the chemical character of the stones that have fallen when they have appeared has been adduced as a proof, that they are "travellers from another planet." The uniformity of their composition appears however to render it probable, that they are always derived from the same source, and their light is in all probability the light of combustion.

Arguments for the smaller being electrical.

Falling stars are evidently a more simple phenomenon. The arguments in favour of their electrical origin are numerous, as will be apparent from the following summary of the principal facts.

1st. Their light is similar to the light of the electric spark.

2d. Their motion, like that of electricity, is inconceivably rapid.

3d. They occur as frequently as other electric changes in the atmosphere.

4th. Their occurrence is most frequent after such changes of weather, as are known to influence the electrical state of the atmosphere.

5th. Their direction is never constant; they move vertically, horizontally, and at various degrees of inclination, in all parts of the atmosphere; such is also the case with lightning.

6th. The appearance of falling stars may be accurately imitated by electricity; and the circumstances on which the success of such experiments depend are such as are likely to occur in the production of the natural phenomenon.

Objections to the planetary hypothesis.

If more powerful evidence can be adduced in support of the planetary hypothesis, it may be intitled to consideration: in its present state it is mere conjecture; and, as opposed to facts, and to analogy, must be considered inadmissable according to the strict principles of experimental inquiry. A few of the circumstances opposed to it may be thus stated:

1st. The *number* of falling stars, and their *frequent*, but not *constant* appearance.

2d. The rapidity of their motion.

3d. Their transient duration.

4th. Their occurrence in a cloudy state of the atmosphere.

5th. Their occurrence when the bright light of the moon renders many small stars and planets invisible.

6th. Their appearance in the lower as well as higher strata of the atmosphere.

From the extensive observations I have made many other particulars might be stated, but I trust what has been advanced will convince your correspondent of the fallacy of his hypothesis, and I cannot but lament he should have stated with such apparent satisfaction the discontinuance of a reference to these phenomena in Mr. Foster's valuable meteorological observations; agreeing, as I imagine every candid inquirer will, in the propriety of Mr. Foster's remark, "that "it is only by repeated and accurate observations of a "multitude of phenomena, the science of meteorology can "be brought to its required perfection."

I remain, sir,
With great regard, yours, &c.
Princes street, Cavendish square, G. J. SINGER.
August the 10th. 1812.

VI.

Sketch of the Geology of Madeira: by the Hon. HENRY GREY BENNET. *In a letter addressed to* G. B. GREENOUGH, *Esq., F. R. S. Pres. G. S*[*].

THE following notes were taken during a short stay I made last summer in the island of Madeira. As there appears to be but little known of the structure, or of the phe-

Geology of Madeira little known.

nomena

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[*] Trans. of the Geolog. Soc. vol. I, p. 391.

Its vallies.

nomena which the strata in that island exhibit, the following observations may not perhaps be wholly unacceptable. They may be considered as furnishing directions to others, where to look for some of the most interesting objects; and may afford to future travellers a small portion of the information, which my guide, Dr. Shuter, so liberally communicated to me. That gentleman, having long resided in the island, had repeatedly traversed it, and was thereby able to point out to me some of the circumstances which were most worthy of examination, particularly the nature of the various strata that are exposed to view in the deep and abrupt vallies which intersect the island in all directions. These vallies are no less picturesque to the eye of the common traveller than they are deserving of the attention of the geologist. They are in general narrow and deep, the summits of the hills that form their boundaries are broken into peaks, rugged and bare, while their sides are covered with the cedar and other trees peculiar to southern latitudes, and with a profuse variety of shrubs and plants, among which the erica arborea is the most beautiful, and in the greatest quantity.

Size of the island.

The island of Madeira (though I believe it never has been surveyed) is said to be about 60 miles in length, and in its broadest part about 20, but the average breadth does not exceed 15 miles.

Hills.

Vallies.

Surface clay.

Extinct volcanoes.

It consists of a succession of lofty hills rising rapidly from the sea, particularly on the eastern and northern extremities. The summits of many of these ranges present the appearance of what has been called a table land; yet occasionally the forms are conical, and surmounted by a peak, which in some instances I found to be of columnar basalt. Deep ravines or vallies descend from the hills or serras to the sea, and in the hollow of most of them flows a small river, which in general is rapid and shallow. The soil of the island is clay on the surface, and large masses of it as hard as brick are found underneath. Though there are not at present any existing volcanoes in the island, yet the remains of two craters are to be seen, one on the eastern, the other on the western side, the largest being about a Portuguese league, or four English miles in circumference. Every thing around wears marks of having suffered the action of fire, yet I was unable to dis-

cove

cover any deposit of sulphur, and was told that none had hitherto been found in the island.

Varieties of lava in strata.

The varieties of strata, which I shall term generally lava, are not numerous. I myself saw but four, and I was informed there are no more to be met with. Three of them were invariably alternating in the same order. The first or lowest lava is of a compact species, containing few, if any, extraneous substances, is of a blue colour, and of a remarkably fine grain. Upon that, the second, which is a red earthy friable lava, rests; sometimes separated by beds of clay mixed with pumice, and layers of black ash and pumice. This red lava contains minute pieces of olivine; sometimes it assumes a prismatic form, and in one place was of a moderate degree of hardness: the principal springs of water in the island issue from this stratum. On the top is the third, a grayish lava, generally compact, though at times near the surface very cellular, and containing much olivine. This lava takes principally the prismatic form of basalt. I have seen it in the most perfect prisms from 30 to 40 feet or more in height, the surface being covered with scoria, ash, and pumice. These masses of lava contain more or less of what I consider to be olivine, occasionally carbonate of lime and zeolite, which last assumes either a crystallized or globular form, or is diffused in a thin coating between the different layers.

A distinct stratum.

The fourth species of lava is of a coarse grain, is used for the making of walls, and the commonest and poorest houses are built of it, the blue and gray lavas being used for the copings, &c. It works easier than the two other kinds abovementioned, is more friable and soft, and its colour is a mixture of brown and red. I observed it in a stratum by itself, and it did not seem to have any connection with the other three kinds.

Varieties not in strata.

These are the principal stratified lavas that the island affords, but in the beds and rivers, particularly in that which flows in the valley of the Corral, several varieties occur in isolated masses, containing olivine and zeolite in greater or less quantity, and exhibing detached portions of strata, similar to those that are found in the fossa grande on the side of Vesuvius.

Columnar basaltes.

Dykes.

Valley of the Corral.

In the deep and singular valley called the Corral, which I had an opportunity of examining for several miles, the red and gray lava alternated five or six times. The tops of some of its barrier hills are formed of columnar basalt; here and there rising to a peak, or broken into what might be termed a crystallised ridge, or tapering to a point like the granite needles in the Mer de Glace. The columnar strata are found here in all directions. They dip usually to the sea, but occasionally are dislocated in the most abrupt manner. Dykes of lava, rising perpendicularly to the horizon, intersect the strata at right angles. I saw one 200 or 300 feet in height, which cut through several of the alternations of the red and gray lava. This valley of the Corral well merits the most attentive examination; yet the journey there is one of some labour, and the walk down the river that flows in its bottom so difficult and toilsome, as almost to deter every one from the undertaking. We left the town of Funchal soon after day break, and did not return till between eight and nine at night, having been, during the whole of that period, in a state of incessant exertion on horseback or on foot. The bed of the valley itself cannot be descended on mules or on horseback. The walk is eight or nine miles in length, and you are compelled to clamber over rocks, as there is not even a track, or wade, in the bed of the river, which is rapid, and full of large and pointed stones. Some of the highest hills of the island border on this valley. Several of them rise from the bed of the river in a perpendicular height of 1000 or 1500 feet, judging only by the eye, and are what the French term taillé à pic. Others are broken into a succession of steep descents, and are covered with forests of wood and a profusion of plants. Down many there fall small cataracts of water, and some are hollowed into deep recesses, whence issue from the lava numerous little streams that contribute to swell the principal river in the valley.

Wall of lava.

As you arrive on the brink of the Corral, after a ride of about 10 miles from Funchal you find yourself suddenly on the edge of a precipice, near to which a sort of traversing stair-case is cut, with a track winding to the bottom. On the right is a wall of lava nearly perpendicular from 400 to 500 feet in depth, composed of the two species of the red and gray, alternating five or six times, and assuming in its dislocation the form of a bow, both the lavas following in a regular bend the shape of the curve.

Lava in very small columns.

On the left of the stairs by which you are to descend, innumerable small columns of the grey lava project from the side; they dip N. W. and their form in general is quadrangular; but I found several of them in prisms of three, five, and six sides. They are remarkably small, and as they lie in this bed, appear almost all to break off from each other at five or six inches in length, and I never found them exceed this size. They seem to form a dyke that cuts through the horizontal beds of lava.

Basaltic columns.

Volcanic products.

At the edge of the descent there is a projection or range of basaltic columns, rising like a wall, tapering to the top, and separating into large quadrangular prisms. We found no black ashes in the valley of the *Corral*, though toward the bottom there are considerable strata of pumice, great masses of scoriæ, and cellular lava, and lava in a state of semi-vitrification; the whole presenting evident marks of an eruption, anterior to that which had formed these various strata of lava, which are visible from the summit of the hill to the bed of the river.

Dip of the strata.

Breccia.

The dip of the strata is in general toward the sea. Basaltic columns shoot from the side of the ordinary strata, which are intersected by various dykes; and one of these in particular swept across both sides of the valley. There are here also rocks of about 100 feet in height, composed of a species of breccia. We examined one near the church, at the extremity of the winding staircase, forming the descent into the valley, which was composed of large and small pieces of lava, some of them of many yards in length and depth, the angles being rounded, and the whole agglutinated together by a hard black earthy substance, that resisted all the force we could use to break off a piece of it. There are other rocks where the red lava forms the base, and these are soft.

Nodules of lava in concentric layers.

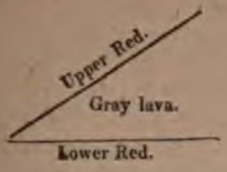
On our road from Funchal to the *Corral* we saw a stratum of large nodules or balls of lava, composed of concentric layers similar to the coat of an onion, and lying one above another; the stratum exposed was 30 or 40 feet in depth, and appeared to go down to the bottom of the hill.

Coast to the west of Funchal.

We also examined the coast to the weetward of the town of Funchal. From the beach before the town to Illhoo Castle, and beyond it to the land called the *Punta de la Cruz*, the general character of the coast is as follows: the red stone is the apparent base upon which rests a bed of gray prismatic lava, the stratum being sometimes from 40 to 100 feet in depth. At times this gray lava rests upon a deep bed of ashes and pumice, agglutinated together like the *peperino* and *puzzolano* in the vicinity of Naples. The scoria at the surface is remarkably thick, and all the upper parts of the lava appear to be cellular. The general dip of the lava on the coast near Funchal is to the north, but near the fort of Illhoo, it forms with a mass of pumice, that is intersected with slight veins of carbonate of lime and zeolite, a rapid angle or curve of declination to the east. To the westward of the fort the lava is not found for a little distance, and there is nothing but deep beds of pumice and the agglutinated mass abovementioned. These beds of pumice are of various thickness, the deepest appearing to be about 4 feet, and alternating with that stratum which I have called *peperino*. In different cavities of the pumice bed, there are large deposits of black ashes. Toward the extremity of the strata the red stone appears on the surface in a more solid state, and lies in prismatic masses, the prisms being small, and not exceeding a few inches in diameter. Their substance is brittle and crumbles with ease. This stratum of red lava is of a short continuance. Passing a small brook, it dips rapidly to the westward, and in its place, the gray lava is found in a confused though sometimes prismatic form, and rises from the beach while the red lava still runs along the surface to the height of near 100 feet, the top being covered with a thick scoria.

Cascade to the east.

There is also in the vicinity of Funchal, to the eastward of the town, a fall of water, which, independent of the romantic beauty of the situation, merits being visited on account of the exposure of the two strata of lava in their relative position. The hills are composed wholly of lava, sometimes of a prismatic formation, the red and gray lavas being visible on both sides of the valley. Near the head of it, a short distance from the cascade, the red stratum is at the bottom, and about 60 feet higher it reappears, and again, about 200 feet higher, alternating with the gray lava. The upper red lava dips rapidly to the south, and the strata are disposed in the following manner.



The rock, down which the cascade falls, is also intersected with a red stratum of about 3 feet wide, that traverses it, and dips to the westward, and is broken off by a broad dyke of gray lava. It appears about 30 feet higher, and dips again to the westward. The substance of the red rock in this place is hard, and it breaks into a columnar form, being by far the most compact of the red strata I met with in the island. I saw this red lava also in the island of Teneriffe, to the eastward of Santa Cruz, as well as in the neighbourhood of Orotava.

The island demands farther examination.

I have thus endeavoured to give you a slight sketch of that which appeared to me most deserving of attention in the island of Madeira. The short stay I was able to make there prevented a more accurate survey of the island; yet I saw enough to induce me to recommend a careful examination of the strata to those who may have more time than I had to spare, and more knowledge to estimate the value of that which was to be seen. To my mind the most interesting geological facts are: 1st, The intersection of the lava by dykes at right angles with the strata. 2dly, The rapid dips the strata make, particularly the overlaying of that of the *Brazen Head*, to the eastward of Funchal, where the blue, gray, and red lavas are rolled up in one mass, and lie in a position as if they had all slipped together from an upper stratum. 3dly, The columnar form of the lava itself reposing on, and being covered by, beds of scoriæ, ashes, and pumice, which affords a strong argument for the volcanic origin

Most interesting facts observed.

origin of the columns themselves; and 4thly, The veins of carbonate of lime and zeolite, which are not found here in solitary pieces as in the vicinity of Etna and Vesuvius, but are *amid* the lavas and *in* the strata of pumice and tufa, and are diffused on the lava itself, and occasionally crystallized in its cavities.

VII.

On the Decomposition of Sulphates by Heat: *by* Mr. GAY-LUSSAC, *Member of the Institute*[*].

Effects of heat on sulphates not such as commonly supposed.

THE object of Mr. Gay-Lussac, in the paper of which I am about to give an abstract, was to make known the effects of heat on sulphates; and the experiments he made for this purpose led him to results very different from those, that others had hastily promulgated on no better ground than probability. Beside extending our chemical knowledge, this paper has the advantage of presenting immediate applications to metallurgy, one of the sciences, to the promotion of which this Journal is devoted. It is in this point of view I shall present the labours of Mr. Gay-Lussac; and, that their application may be more obvious, I shall take the liberty of making a little change in the order of his experiments.

Roasting of sulphuretted ores.

He observed the phenomena that accompany the roasting of several metallic sulphurets, as well as the results of that operation: and his experiments farther confirm the opinion, that the formation of sulphates is unavoidable in the roasting of sulphuretted ores; and that the separation of the sulphur is not complete, till these sulphates have been decomposed. I shall first give his experiments on this subject, and then proceed to the decomposition of the sulphates.

Sulphate of copper made by roasting the sulphuret.

"I knew," says Mr. Gay-Lussac, "that in several manufactories sulphate of copper is made by roasting the sulphuret in reverberatory furnaces. At Goslar sulphate of zinc is prepared by a similar process. I attempted to imitate this process in the small way, and succeeded completely. Repeating it on sulphuret of iron, and on a mixture of sulphur and black oxide of manganese, it still afforded me sulphates. The temperature at which these sulphates were roasted was a red heat scarcely visible."

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[*] Journal des Mines, vol. XXII, p. 325. Taken from the Mém. of the Society of Arcueil, vol. I.

Sulphuric acid formed in roasting alkaline as well as metallic sulphurets.

.... "The formation of sulphuric acid in the roasting of metallic sulphurets is not peculiar to them. It takes place also, and much more decidedly, in the roasting of alkaline sulphurets. I made some sulphuret of potash, which remained fluid at a low red heat as long as it was kept from the contact of air: but as soon as air was admitted freely, it began to thicken; and soon after it became solid, because a great deal of sulphate was already formed. I removed it from the fire to powder it, and exposed it anew to the action of heat. In less than an hour it had lost its sulphurous taste, and threw down only a white precipitate with acetate of lead. The sulphuric and muriatic acids extricated nothing from it. The sulphuret of barytes, treated in the same manner, likewise afforded me sulphate; but after three hours roasting in a red heat it was still sulphuretted. I examined these two alkaline sulphurets, and several metallic sulphurets, at various periods of roasting, without ever being able to extricate sulphurous acid from them. Consequently they must pass directly to the state of sulphates.

They pass directly to the state of sulphates.

Reason of this.

"It is easy to understand why the alkaline sulphurets pass immediately to the state of sulphates in roasting; for Mr. Berthollet has shown, in the Memoirs of the Academy, that the sulphite of potash is converted into sulphate at a red heat, exhibiting an excess of sulphur and of alkali. On treating sulphite of lead in the same way I obtained a great deal of sulphurous acid; which proves, that the oxide of lead has a much weaker action than potash on sulphuric acid. It is probable however, that some sulphate is formed with this oxide also; and if I cannot absolutely affirm this, it is because what I found in the residuum might proceed from sulphuric acid contained in my sulphurous acid.

Oxide of lead acts more feebly than potash on sulphuric acid.

All metallic sulphurets not alike in the formation of sulphuric acid.

"All the metallic sulphurets however are not equally adapted to produce sulphates by roasting. A necessary condition for the formation of sulphuric acid is its having a base to combine with capable of condensing it sufficiently. I have taken

Sulphuret of tin,

antimony, bismuth,

and silver.

sulphuret of tin, a metal that does not combine with sulphuric acid but very difficultly; and I have roasted it in a red heat for an hour, without any thing but sulphurous acid being produced. In like manner the sulphurets of antimony and bismuth, after having been roasted, presented me only with traces of sulphuric acid. It may be remembered too, that, if sulphates of these different metals be distilled, almost all the sulphuric acid passes over, as if it had been alone. The affinity of the metal for oxigen also has some influence. When sulphuret of silver is distilled in a stone retort with a strong fire, it is not decomposed: but if it be roasted, it decomposes with the greatest facility, sulphurous acid only is evolved, and the acid is not oxided.

Important circumstance, the condensation of the acid.

"Thus, then, an important circumstance, the condensation of the acid, modifies the phenomena presented by the metallic sulphurets in roasting. When the metals have the property of combining with sulphuric acid, and causing it to undergo a certain degree of condensation, sulphates are always formed. When, on the contrary, they combine but very difficultly with it, sulphurous acid only is formed, which flies off, as its great elasticity cannot be overcome by the affinity of the metallic oxides."

We will now proceed to the decomposition of the sulphates, which is the principal subject of the paper.

Common theory of the distillation of sulphates

erroneous.

......"It was supposed," says Mr. Gay-Lussac, "that, on distilling a metallic sulphate, sulphuric acid was obtained, if the oxide were not susceptible of a farther degree of oxidation; or sulphurous acid, if its oxidation might be carried farther. It was thought too, that all the alkaline and earthy sulphates with excess of acid were brought back to the neutral state by the action of caloric, or entirely decomposed, yielding as the result only sulphuric acid. This theory is not the expression of facts accurately observed."......

Effects of heat on sulphate of copper.

......"The first sulphate subjected to the action of heat was that of copper. Water first passed over: but as soon as the retort began to grow red, white vapours of sulphuric acid arose, accompanied with a nebulous gas; smelling strongly of sulphurous acid, and in which a match kindled several times following, when it had been washed. This gas therefore was a mixture of sulphurous acid and oxigen. As the distillation proceeded, it appeared to me, that the quantity of sulphuric acid diminished in regard to the oxigen gas and sulphurous acid; and consequently that less acid escaped decomposition than at the commencement of the process. When nothing more was given out, I removed the retort from the fire. The oxide had not been fused, and it retained some acid; which proves, that at a higher temperature it would have been decomposed completely. The sulphurous acid and oxigen gas arose necessarily from the immediate decomposition of the sulphuric acid. The oxide of copper dissolved in fact in nitric acid without effervescence: and it is known, that it does not acquire a higher degree of oxidation in the distillation of its sulphate. The two gasses were to each other in bulk nearly as two to one."......

Decomposition of sulphate of iron by heat.

" Though sulphuric acid has long been prepared by distillation from sulphate of iron, and this has been an object of continual examination, attention has not been paid to several circumstances presented by its decomposition. It was known, it is true, that the sulphuric acid was always accompanied with sulphurous acid: but as the iron takes a higher degree of oxidation in this process, it was supposed to give rise to all the sulphurous acid by decomposing the sulphuric. Mr. Chaptal was, I believe, the first, who remarked, that a little oxigen also was obtained. In fact, the sulphate of iron undergoes the same decomposition by heat as the sulphate of copper: only the results are modified by this circumstance, as the metal is susceptible of a higher degree of oxidation, the proportion of sulphurous acid to oxigen gas evolved is greater."

Sulphates of manganese and zinc.

" The sulphates of manganese and zinc have exhibited to me precisely the same phenomena as the sulphate of copper; and therefore I shall not stop to describe them. I shall only observe, that the first of these salts may easily be prepared by calcining the black oxide of manganese in a red heat; for after this it dissolves readily in sulphuric acid."

Two sulphates of tin, antimony, & bismuth.

" When concentrated sulphuric acid is made to act on tin, antimony, or bismuth, two compounds are formed. One, which is very soluble, retains a great deal of acid, and very little oxide: the other, on the contrary, is formed of much more oxide than acid, and has little solubility."

Action of heat on them.

"If the first of these compounds be distilled, the sulphuric acid is volatilized as if it were alone: but if the second, in which the sulphuric acid is more strongly retained, be subjected to distillation, oxigen gas and sulphurous gas are evolved."

Products of sulphates retaining the acid in weaker degrees.

"The salts that have hitherto been examined have yielded different products, according to the strength with which the sulphuric acid is combined in them. When it is feebly retained, and has undergone no condensation, it is volatilized by heat as if it were alone, without being decomposed. If it be retained more forcibly, part only escapes decomposition, and the other part is converted into oxigen and sulphurous acid gasses. The insoluble sulphates, in which there is no sign of acidity, appearing to retain the acid with great force, it is essential to know what is the action of heat on them."

Those that retain it very forcibly examined.

Sulphate of silver.

"I put some sulphate of silver into an uncoated glass retort, furnished with a tube for collecting the gas. When it began to grow red, the salt melted, but was not decomposed. Having taken it out, I exposed it to a more violent fire in a stone retort; and then a great deal of oxigen gas was evolved, mixed with sulphurous acid, as Mr. Fourcroy announced. I did not perceive any dense white fumes as in the preceding experiments, because very little sulphuric acid was given out. When the process was finished, I found in the retort a button of silver completely reduced. Thus the sulphate of silver is decomposed by heat like the other sulphates, but it gives out more oxigen than they; on the one hand, in consequence of the reduction of the metal, on the other, because it yields very little sulphuric acid."

Sulphate of mercury.

"I afterward prepared some sulphate of mercury, by precipitating nitrate of mercury little oxided with sulphate of soda. The precipitate, washed and dried, was exposed to heat in an uncoated glass retort. Scarcely had this begun to grow red, when the salt entered into fusion, and it was soon decomposed. Very little sulphuric acid passed over; and mercury sublimed, with a little sulphate. The other products were sulphurous acid and oxigen gasses mixed in the proportion of 51·5 to 48·5. Though oxide of mercury requires a higher temperature for its reduction than the oxide of silver, the sulphate of silver is not so readily decomposed as that of mercury. This difference may arise in part, no doubt, from the difference in the affinities of the metals for sulphuric acid; but it must depend likewise on the great volatility of the mercury. In general it appears to me, that the affinity, the more or less easy reducibleness of the metals, and their volatility, must be considered as so many causes, capable of modifying the action of caloric on their sulphates."

Sulphate of lead.

"From my first experiment with sulphate of lead, not having employed a temperature sufficiently high, I concluded, that it was not decomposable by heat. But on having recourse to a reverberatory furnace surmounted with a chimney, I obtained its decomposition, and collected a great deal of oxigen gas and sulphurous acid. I did not perceive any lead reduced, or any very sensible quantity of sulphuric acid. It is very possible, that the separation of the acid was determined by the action of the stone retort, for it was coated internally with vitreous glaze*. Be this as it may, it is evident, that the sulphate of lead, which is insoluble and without excess of acid, and the decomposition of which cannot be promoted either by the easy reduction of the oxide, or by the volatility of the metal, is much more difficultly decomposed by fire than the acid and soluble sulphates. We may conclude therefore, that the insoluble sulphates resist the action of caloric more than those that are soluble, and that they give out much less sulphuric acid. But to render this conclusion still more general, we must take into consideration the more or less easy reduction of the metals, and their volatility."

Difference between the sulphates.

"It may have been observed, that the soluble sulphates yielded more sulphuric acid, than those that are insoluble. When the former have lost a part of their acid, their solublity is diminished, the acid remaining is held with more

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* This suspicion of the author appears to me well founded. The great affinity of the oxide of lead for earths, and particularly for silex, must facilitate the decomposition of the sulphate, if it be not its sole cause.

force

Two portions of acid in metallic sulphates.

force, and then they must approach nearer the second. We can conceive two portions of acid in metallic sulphates: one, which is feebly retained, escapes without undergoing decomposition; the other, which is retained more strongly, supports a more elevated temperature, and is decomposed into sulphurous acid and oxigen gas. These two portions of acid, which we may conceive to exist in sulphates, are not the same in all: and it appears, all other circumstances being equal, that, the more soluble a salt is, and the greater its excess of acid, the more sulphuric acid is obtained in its distillation. Hence it is, that sulphuric acid may be prepared, as is done in Germany, by distilling sulphate of iron or of zinc. The insoluble sulphates would not be any way adapted to this purpose."

Alkaline and earthy sulphates.

Mr. Gay-Lussac has extended his researches to the alkaline and earthy sulphates: and he has found, that the salts with excess of acid comport themselves altogether as the metallic sulphates; that is to say, they give out sulphuric acid, sulphurous acid, and oxigen. Those that do not admit an excess of acid do not yield, even if sulphuric acid be added to them, either sulphurous acid or oxigen gas; and nothing is obtained in distilling them but the acid, that exceeded what was requisite for their neutralization. Such are the sulphates of barytes, lime, &c.

Roasting of sulphurets.

The author, applying all these facts to the roasting of sulphates, concludes...."that, when the roasting is performed at a temperature equal to that at which the sulphates are decomposed, and still more when at a higher temperature, no sulphuric acid will be obtained, and all the sulphur will be given out in the state of sulphurous acid."

Another mode of decomposing sulphates by heat: the addition of sulphuret.

"Beside this mode of decomposing sulphates by heat, there is another, which is more convenient, as it does not require so high a temperature. It is that employed by Mr. Gueniveau to decompose the sulphate of lead, distilling it with the sulphuret of the same metal*. I satisfied myself, that by treating the sulphates of iron and copper in like manner with the sulphurets of the respective metals sulphurous acid only was obtained: which proves, 1st, that in this way we may separate the sulphur from metallic sulphurets and sulphates; 2d, that to effect this separation does not require so high a temperature as is necessary to decompose the sulphates."

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* See Journal, vol. XVIII, p. 203, 204.

"Lastly in distilling a metallic oxide and its sulphuret, a great deal of sulphurous acid is obtained, and a little sulphate: but, if the temperature be sufficiently high, nothing remains but sulphuret, or merely oxide, according to the proportions employed."

Metallic oxide distilled with its sulphuret.

"Now we are acquainted with the various circumstances, that may present themselves in the roasting of a sulphuret, it is easy to give the theory of it. To roast a sulphuret is, as the ultimate result, to separate the sulphur by the the simultaneous action of air and heat. The products obtained vary in general according to the temperature, and to the sulphuret roasted. At an ordinary red heat these sulphurets, the metal of which combines but difficultly with sulphuric acid, yield scarce any thing except sulphurous acid. Those, on the contrary, that condense it strongly, yield also, it is true, sulphurous acid; but at the same time sulphuric acid is produced, which remains combined with the oxides. At a very high temperature, superior to what would be necessary to decompose the sulphates, all sulphurets yield only sulphurous acid. When once sulphate is formed, it may be decomposed by a more powerful action of heat; or still better by those parts of the sulphuret, that have not yet undergone any change. In fine, when other portions have lost their sulphur, and are oxided, they are capable of taking the sulphur from those that still retain it, and converting it into sulphurous acid."

Theory of the roasting of sulphurets.

The author has availed himself of the facility, with which, in the decomposition of sulphates, the sulphuric acid yields its two component parts, sulphurous acid and oxigen, to ascertain the composition of this acid. He has found, that 100 parts by measure of sulphurous acid gas take 47·79 of oxigen gas to form sulphuric acid; and, admitting the proportions given by Klaproth for the sulphate of barytes, he thence deduces the composition of sulphurous acid, which is 100 sulphur to 91·68 of oxigen.

Examination of the composition of sulphuric and sulphurous acid.

Mr. Gay-Lussac afterward describes the ingenious experiment, in which he decomposed pure sulphuric acid simply by

Sulphuric acid decomposed by heat alone.

by heat, by passing it through an incandescent porcelain tube, thus obtaining oxigen and sulphurous acid gas. This experiment explains the decomposition of sulphates.

Different effects of heat on sulphates.

"All the neutral or acid sulphates, that lose their acid at a temperature below that required for decomposing sulphuric acid, will decompose without giving out oxigen or sulphurous acid. Those, on the contrary, that retain all their acid so strongly as to resist a heat equal or superior to that which decomposes sulphuric acid, will give out only oxigen gas and sulphurous acid. Lastly, as a compound does not equally retain every portion of its elements, there are sulphates, the decomposition of which will partake of the two preceding, and which will give out sulphuric acid, oxigen gas, and sulphurous acid."

Effects of the attraction of the base for the acid.

Thus then, setting aside the particular influence of a given base, we should consider the affinity, that unites the sulphuric acid to the base with which it forms a sulphate, as a force that enables it to support without being volatilized a heat sufficient to decompose it; while, if it had been free, it would have withdrawn itself from the action of the heat, long before it experienced the degree necessary for this decomposition. But this force of affinity is likewise an additional obstacle to be overcome by the action of heat: and this obstacle is very considerable, when the base undergoes no alteration by heat, as the oxide in the sulphate of lead, and the fixed alkalis in their sulphates.

The paper contains many other very delicate researches of considerable importance to the chemical theory of several phenomena, but I shall here finish my abstract with the following

General deductions.

"*Conclusion.*

Decomposition of the metallic sulphates,

"1st. All the metallic sulphates are decomposable by the action of heat, affording results that depend on the affinity of the metals for sulphuric acid. The sulphates in which the acid is but little condensed yield only sulphuric acid by distillation. Those that retain it more strongly, and are insoluble, give out sulphurous acid and oxigen gas. Lastly, those that have properties common to both the preceding, and which are acid and insoluble, give out sulphuric acid, oxigen gas, and sulphurous acid.

metallic sulphurets,

2d. In the roasting of metallic sulphurets, the products vary according to the temperature, and according to the sulphuret. At a very high temperature, sulphurous acid is given out in quantities so much the greater in proportion as the oxide is capable of condensing it more strongly; and when it has but a very weak affinity for it, no sulphurous acid is formed.

earthy sulphates,

"3d. All the earthy sulphates, which are naturally acid, are decomposable by fire, giving out sulphuric acid, oxigen gas, and sulphurous acid.

and alkaline sulphates.

"4th. The neutral alkaline sulphates are not decomposable by fire, that of ammonia excepted: but when they are capable of forming crystallizable salts with excess of acid, condensing it, and diminishing its volatility, part of this excess of acid is changed into oxigen gas and sulphurous acid.

Action of phosphoric and boracic acid.

"5th. Sulphates treated in the fire with phosphoric or boracic acid yield sulphuric acid, oxigen gas, and sulphurous acid.

Composition of sulphuric and sulphurous acid.

"6th. Sulphuric acid is composed of 100 parts sulphurous gas, and 47·79 oxigen gas, by measure.

"7th. 100 parts of sulphur by weight require 50·61 of oxigen, to convert them into sulphurous acid, and 85·70 to form sulphuric acid.

Sulphuric acid decomposed by heat.

"8th. Sulphuric acid is decomposable by heat alone into oxigen gas and sulphurous acid gas.

Manufacture of sulphuric acid.

"9th. A great elevation of temperature is not favourable to the production of sulphuric acid; on the contrary, it is detrimental to it. The instant of the combustion of sulphur, sulphurous gas only is obtained, whether it take place in the open air or in oxigen gas; and the sulphuric acid obtained in leaden chambers must be the result of the action of nitrous gas and of the air on the sulphurous acid, as well as of the action the last mentioned gas exerts on oxigen by means of water.

VIII.

Remarks on some useful Applications of Meteorological Observations to Nautical Prophylactics: by F. PERON, *Naturalist of the Voyage of Discovery to the Austral Lands, Correspondent of the Imperial Institute, &c.*[*]

Meteorological observations

METEOROLOGICAL instruments, it is true, are but modern acquisitions to science; yet observations with them have been pursued so steadily, and in so many different climates, that we have reason to be equally astonished at the imperfection of their theory, and of the few useful applications they furnish. Perhaps the chief reason of this is to be found in the nature of the theatre on which these experiments have been made almost exclusively to the present day. In fact, how many joint causes concur, in the midst of our continents, to complicate results essentially so different and so delicate! The observer on the ocean, on the contrary, left to the exclusive influence of the air and water, may give greater precision and developement to his experiments, and deduce conclusions more exact, and more general in their application. It is not my intention here to enter into what I had myself an opportunity of doing in this way amid so many seas, repeating my observations daily at six o'clock morning and evening, at noon, and at midnight; but to confine myself to a few experiments, which appear to me more immediately connected with the health of mariners.

on land

and at sea.

Observations connected with the health of seamen.

In this class I conceive may be placed a series of tables of the variation of the barometer, hygrometer, and thermometer, and of the temperature of the sea at its surface, taken at every hundred leagues for 95 degrees of latitude; an undertaking that appears to me as new, as it is capable of becoming at a future period of importance in preserving the health of seamen. By multiplying tables of this kind, constructed with as much care as I employed in it, we should soon have a kind of *meteorological hydrography*, equally indispensable to the natural philosopher and the physician. The latitude and longitude of a part of the sea being given, we might find by these tables the general state of the atmosphere and water pertaining to it; and thus ascertain its influence on the mariners who traverse it, and the animals that people it.

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[*] Journ. de Phys. vol. LXVII, p. 29.

Chief cause of scurvy.

In my meteorological labours, however, I had in view an object still more essential, and more immediately useful to seamen. Theory and experience, in fact, seem to unite to prove, that the chief, if not the exclusive cause of scurvy is moisture, whether combined with heat, or with cold. This opinion, which Mr. Keranden has particularly unfolded in his excellent dissertation on this subject, and which my own disasters confirmed, led me to consider it as a duty, to direct my inquiries to this subject; and to pursue them with the more care, as I had the advantage of being the first to traverse the seas with an hygrometical instrument capable of being compared with others, that of de Saussure, executed by Richer. Besides, Mr. Hallé, to whose instructions and advice I am so much indebted, recommended observations of this kind to me at my departure; and the desire of testifying my gratitude to him, at least by my zeal, was a powerful motive with me to undertake them.

The author's observations.

Independant of my other meteorological researches, therefore, I imposed on myself the task of making particular experiments on the comparative state of different parts of our vessel. Every ten days, at noon and at midnight, I went from the poop beneath the quarter deck and forecastle*, thence to the gunroom, and lastly to the hold, where I caused myself to be shut up for half an hour, to obtain results more exact, and more accurately comparable. The captain, who had requested me to communicate to him the results, and transcribed them into his journal, always afforded me, as I must candidly confess, every possible convenience for this purpose; and in this respect, at least, he was pleased to second my endeavours.

Information derived from them.

My observations toward the close of october 1800 showed me, that the matter of the vomitings of several persons attacked with seasickness, and too much crowded in the gunroom, had altered the air in a dangerous manner by its decomposition. The tempestuous weather we had experienced for several days not having allowed us to open the ports, this occasioned other inconveniencies, not less serious than those from the cause just mentioned. The thermometer, which without was scarcely so high as 8° [46·4° F.], in the gunroom was at 15° [59° F.]; and the hygrometer rose from 78° to 96°. Lastly, a considerable portion of sulphuretted hidrogen gas evinced its presence, not merely by its peculiar smell, but by the yellow hue almost every article of silver in the place contracted. On the report I made to the captain, the taking down all the hammocks was strictly enforced, the decks were carefully swept, fumigations were repeatedly made, the ports were directed to be opened, a windsail was employed, and in a few days the former salubrity of the gunroom was restored.

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* *Sous le gaillards*, in the original; though, from the table, I suppose the author means the between-decks. C.

Another instance.

In my report of the 21st of november I again apprised the captain, that the excessive heat I observed during the night in the gunroom indicated, that too many persons slept in it; and as this damp and hot temperature could not fail to be prejudicial to all, it was indispensably necessary to remove some of them. The captain reduced the number from twenty-four to fifteen or sixteen, and the results I obtained the following night confirmed the justice of my observations.

On the 11th of december, on going into the hold, I perceived a sour, nauseous, and extremely disagreeable smell; and my candle burned with difficulty. I soon learned, that a cask of wine had been leaking for some days; so that I had no difficulty in accounting for the smell, and for the great proportion of carbonic acid gas. I hastened to acquaint the captain with this; and recommended pumping the ship dry, throwing fresh water into the well, and pumping it out repeatedly. Orders were immediately given for this purpose, and the ship was once more rendered sweet by my advice.

A third.

My experiments at the close of december afforded me a triumph peculiarly flattering, as they served evidently to prove the importance of meteorological observations on board ships. The storeroom of the captain and principal officers was filled with all sorts of provision put on board in Europe; fruit dry and preserved, *adaubages* in large quantity, fats, oils, &c. On going into it with my instruments, I was equally surprised and grieved at the results they offered me: and I gave an account of them to the captain in the following words.

Report to the captain.

"A noisome smell, and excessive heat and moisture, conspire to render the storeroom unwholesome. On attempting to make my usual experiments there, I found myself so ill and faint, that I could not finish them. My thermometer however had already risen to 27° [80·6° F.], and the hygrometer was beyond the point of saturation. The flame of the candle was feeble and pale, indicating the presence of a great quantity of gas unfit for respiration. It is true no one lives in the storeroom: but is there not reason to fear, that such of the men as are obliged to work there will soon feel its fatal effects? It appears to me, therefore, indispensably necessary, to empty this place for a few days, and to endeavour by fumigations, sprinkling with cold water, ventilating, and repeated sweeping, to renew the air, and remove its humidity. This precaution is as necessary for preserving the provision, as for the health of the men: for there can be no doubt, that many articles are already spoiling, and others will soon be so, from the high temperature and extreme moisture combined. At any rate, if the nature of the service will not allow these means to be employed, it is to be wished, 1, that the men were forbidden to go alone into the storeroom; not only to prevent suffocation, of which there are but too many instances in similar cases; but to obviate the more fatal effects that might follow, should such an accident take place, from the person's being left, or from the candle: 2, that the men should have their allowance of wine increased one fourth; for it is to be feared, that, coming out of the storeroom in a profuse perspiration, some accident might happen from their drinking a large quantity of water to quench the thirst produced; an effect I could not avoid myself, notwithstanding the short time I staid, and my remaining almost perfectly still."

Neglected from the assertion of an officer.

The captain, alarmed at this report, immediately sent for the officer, under whose care it was, and communicated it to him. He asserted, that it was altogether erroneous, that the observations were of no consequence, that the stores were in good condition, &c. Accordingly nothing was done: but though their advantages had been sufficiently proved.

sire of doing good is not always sufficient. I consoled myself however by reflecting, that I had acquired a certainty of the advantages of meteorological observations on board ships; and I am still firmly persuaded, that the continuation of similar labours, and the particular inspection to which they would have led, would have been of great service during the rest of the voyage; and though they would not have prevented the dreadful scurvy, that made such ravages among our crew, they would perhaps have checked its progress. The little good I was able to accomplish, while it proves the utility of such experiments, will no doubt stamp a due value on the counsels of our naval officers of health, and may thus contribute to the improvement of nautical medicine, too little acquainted hitherto with the assistance it may derive from that application of natural philosophy to the healing art, which Mr. Hallé has so successfully pointed out.

Advantages of having meteorological instruments on board all ships.

What for instance can be more easy, and at the same time more necessary, than to place in the care of the surgeon of every ship a good marine barometer, a few thermometers, and a couple of hygrometers? What series of valuable observations on the constitution of all the climates on the globe, and what important materials for nautical medicine and the science of natural philosophy, would thus be acquired at a trifling expense! How advantageous also would these instruments be to mariners themselves! I do not speak merely with respect to a more accurate judgment of changes in the atmosphere, which the barometer and hygrometer would frequently furnish, and which established the reputation of these instruments with the officers of our expedition; but with regard to their health, and its preservation. Beside what I have already said, how often, for instance, at an anchorage, or when tents were pitched ashore, the changes of the weather having been shown to be dangerous by our meteorological instruments, might the crew have been preserved from their effects at a trifling expense, and without inconvenience! Thus at the head of the Bay of Seals, where I observed variations of 20° [36° F.] of temperature and 33° of humidity within the 24 hours, those of the crew of the Naturaliste who slept on shore being almost all attacked with a violent diarrhœa, need we seek any other cause for it than the

Setting up tents ashore.

the daily and alarming vicissitudes of the weather? And when the results of our meteorological observations had pointed out the true etiology of this kind of epidemic, would they not have led a sagacious observer to the means, equally simple and efficacious, which the natives of this coast, afflicted no doubt by such fatal changes, have contrived to diffuse around them, to obviate their dangerous effects? means which were probably the fruit of too long experience, and too long misfortunes to these rude people*.

Ordering up hammocks.

Regulations of diet.

Washing decks.

By the assistance of the same instruments how often should we be led to act with more caution in regard to exposing the seamen to the weather, and the daily practice of ordering up all the hammocks! How often might we not introduce with equal advantage and facility some salutary variations either in the distribution of the provision, or in the succession of the various aliments, with which a ship is furnished! On seing daily the thermometer sink instantaneously several degrees, and the hygrometer indicate 8° or 10° of additional moisture, precisely when, by order of the captain, the deck, forecastle, quarterdeck, and great cabin, had just been sluiced with sea water for the purpose of cleaning them, what commander, less opinionative than ours, but would have been eager to put a stop to such a fatal practice? what officer but would have preferred simple dry scraping to those monstrous ablutions with salt water, which daily filled the interior of the ship with a damp and cold atmosphere, and in my opinion contributed not a little to the rise of that terrible epidemic scurvy, which destroyed our crew on the coasts of Napoleon Land, and Van Diemen's Land?

Attention to little circumstances important.

They who are unacquainted with the minutiæ of long voyages may think most of these precautions useless: but if they reflect on the importance attached to them by the most celebrated navigators, and particularly by the most successful, they will be convinced, that an attention to a multitude of little things, apparently indifferent, especially if considered singly, form the essential basis of that science of preserving the health of seamen, sanctioned by the valuable success of

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* For an explanation of this passage, see chap. XXX of the Narrative of our Voyage, where I have described the singular habitations of the people of Endracht's Land.

Bougainville, Cook, Vancouver, and Marchand. In the ship of the last of these in particular the art of preventive medicine displayed in a striking manner what may be expected from these little attentions. Mr. de Fleurieu, in his account of the voyage I have just mentioned, has given a just eulogium of the surgeon of the *Solide*, Mr. Roblet; and when I was at the Isle of France I had an opportunity of becoming acquainted with this gentleman, and receiving from him a confirmation of the useful hints here given for the improvement of nautical medicine, which is so greatly indebted to him. His happy employment of warm sand baths for the cure of the scurvy at sea, and the striking success with which it was attended, confirming that of Mr. Bellefin, surgeon of the *Naturaliste*, must render his name dear to every lover of the art and friend of mankind.

While paying to this gentleman, equally learned and modest, the tribute of praise due to him, I cannot avoid noticing a remarkable expression of Vancouver's, well adapted to show the importance of such services, too little known and too soon forgotten. After having spoken of the improvement of this branch of physic, which he ascribes particularly to the beneficient genius of Cook, he adds: "it is to this *inestimable* improvement, that Britain is *in great measure* indebted for the high rank she at present holds among nations."

If we must learn the principles of preserving the health of seamen from a nation, to which men are so valuable, because its population is so greatly disproportionate to its establishments; it belongs to the celebrated Society*, to which I have the honour of addressing myself, to make them known, and render them useful to our country.

Table of Experiments made to ascertain the relative proportions of humidity in different parts of the ship le Geographe.

Meteorological observations at sea.

Oct. the 22d, 1800, at noon, lat. 49° 36′ N., long. 6° 44′ W. [4° 24′ W. from London], after several days of tempestuous weather, that did not allow the ports to be opened in any part of the ship.

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* The Medical School of Paris, in whose memoirs this paper is inserted.

On

Meteorological observations at sea.

	Thermometer. Cent.	Thermometer. Fahr.	Hygr.
On the poop	8·5°	47·3°	78°
In the gunroom, the ports shut	14·5	58·1	96
Oct. the 23, at noon, lat. 48° N., long. 8°43′ W. [6°23′] the cessation of the tempestuous weather having allowed the ports to be opened, and the different parts of the vessel to be cleaned.			
On the poop	11·5	52·7	85
In the gunroom	13	55·4	89
Nov. the 1st, at 8 A. M., in sight of the island of Teneriffe.			
On the poop	16·5	61·7	78
In the gunroom, the ports open	17·5	63·5	81
Under the between-decks	18·5	65·3	85
In the hold	18·9	66·02	90
Nov. the 19th, at 8 A. M., lat. 13° N., long. 22° W. [19° 40.]			
On the poop	21	69·8	93
In the gunroom, the ports open	22	71·6	94
Under the between-decks	22	71·6	96
In the hold	24·5	76·1	98
Nov. the 22d, at noon, lat 8° N., long. 20° W. [17° 40′].			
Poop	24·5	76·1	90
Between decks*	24·4	75·92	94
Gunroom	24·9	76·82	92
Hold	22·6	72·68	97
Nov. the 30th, at midnight, lat. 6° 38′ N., long. 19° W. [16°40′].			
Poop	22·6	72·68	92
Between-decks	23	73·4	93
Gunroom	24	75·2	96
Hold	21·5	70·7	96
Dec. the 10th, at noon, lat. 2° N., long. 20 W. [17°40′].			
Poop	21·8	71·24	93
Between-decks	22·5	72·5	98
Gunroom	22·3	72·14	96
Hold	23·7	74·66	101

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* Here and throughout the remainder of the table, the expression is changed by omitting the preposition: but, as it is omitted equally for the other parts, I suppose the author means the same here by *entrepont* simply as before by *sous l'entrepont*. C.

The

Meteorological observations at sea.

	Thermometer. Cent.	Thermometer. Fahr.	Hygr.
The same day at midnight, lat. and long. the same.			
Poop	19·8°	67·74°	97°
Between-decks	23·6	74·48	100
Gunroom	22·6	72·68	97
Hold	24·5	76·1	103
Dec. the 21st, at noon, lat. 11° S., long. 31° W. [28°40′].			
Poop	21	69·8	91
Between-decks	21·4	70·52	95
Gunroom	21·8	71·24	92
Hold	23	73·4	100
The same day at midnight, lat. and long. the same.			
Poop	20·2	68·36	91
Between-decks	22	71·6	96
Gunroom	21	69·8	90
Hold	23·2	73·76	103
Dec. the 30th, at noon, lat. 23° S., long. 26° W. [23°40′].			
Poop	19·1	66·38	90
Between-decks	20	68	92
Gunroom	19·6	67·28	91
The same day at midnight, lat. and long. the same.			
Poop	18	64·4	92
Between-decks	19·3	66·74	95
Gunroom	19·7	67·46	94
Jan. the 10th, 1801, at noon, lat. 30° S., long. 21° W. [18°40′].			
Poop	18·5	65·3	81
Between-decks	19·8	67·64	85
Gunroom	18·9	66·02	84
The same day at midnight, lat. and long. the same.			
Poop	16·4	61·52	88
Between-decks	20·2	68·36	89
Gunroom	17·4	63·32	90
Jan. the 20th, at noon, lat. 33° S., long. 3° W. [0°40′].			
Poop	15·5	59·9	80
Between-decks	15·8	60·44	85
Gunroom	16	60·8	83
Hold	21·2	70·16	92
The same day at midnight, lat. and long. the same.			
Poop	13·8	56·84	79

	Thermometer.		Hygs.
	Cent.	Fahr.	
Between-decks	16·5°	61·7°	87°
Gunroom	15·4	59·72	78
Jan. the 30th, at noon, lat. 35° S., long. 7° E. [9°20′]			
Poop	16·6	61·88	92
Between-decks	16·7	62·06	91
Gunroom	15·9	60·62	89
The same day at midnight, lat. and long. the same.			
Poop	14·7	58·46	102
Between-decks	17·3	63·14	98
Gunroom	17·6	63·68	96

General results.

General conclusions.

On comparing these different observations we find, with respect to the temperature,

Temperature.

1, That the temperature of the air in the interior of the vessel is generally 3° or 4° [5·4° or 7·2° F.], higher than that of the exterior air.

2, That the difference of temperature between the gunroom and the between-decks was scarcely 1° [1·8° F.], when, by opening the parts and employing windsails, care was taken to keep up a salutary current of air in the gunroom.

3, That, circumstances being the same, the hold of the ship is the hottest part. The exceptions to this rule appeared to me to correspond with its being washed out, which was done by introducing into it repeatedly large quantities of water, the happy effects of which were both to clean and to cool this place.

Moisture.

With respect to *moisture* we find from the preceding experiments,

4, That there is habitually more dampness in the vessel, than in the open air. The few exceptions to this rule depended on slight variations in the atmosphere, by which the outer air was naturally affected sooner than that within the ship.

5, That the difference of moisture between the air within and without the ship is generally greater than that of the temperature, it frequently amounting to 10° or 12°.

6, That, circumstances being the same, the gunroom is less damp than the between-decks, and this singular result appeared to me to be owing entirely to those fatal inundations which were employed daily between decks, while the gun-room was cleaned dry, the vicinity of the powder preventing the introduction of water there.

7, Lastly it follows from the experiments, that, if the hold be the hottest part of the vessel, it is also the dampest, and that on both accounts it ought to be considered as the most unwholesome.

IX.

Account of the Vicuna: by Mr. LARREY, *Physician in Chief of the Imperial Guard, one of the Inspectors General of Military Hospitals, &c.*[*]

Two young vicunas brought to Spain.

A Merchant of Cadiz, a lover of natural history, brought from Peru two young vicunas, a male and female. He first landed them at Cadiz at the beginning of the year 1808; and toward the end of April in the same year conveyed them to Madrid. They did not appear to be inconvenienced by the change of climate, or difference of food, till the weather began to get very hot. They were very badly lodged in a small, dark room, not well ventilated. In this hole I had an opportunity of seeing them, examining their figure and gait, and studying their manners and habits.

Death of the female,

The female, which was larger and older than the male, being about three feet high, died soon after, during a short tour I made in the neighbourhood of Madrid to inspect the hospitals. I could not learn the cause of her death; but, as the body quickly putrefied, it was thrown into the fields.

and soon after of the male.

On my return I hastened to visit the two strangers, but found only the male, sad, dejected, and uttering plaintive cries at the slightest touch. He ate but little, and remained constantly squatted on his four legs: but he appeared better and more lively in the cool of the evening and morning, which he seemed to seek; while in the heat of the day he was overcome, and breathed with difficulty. Thus melancholy and unwell he passed the first week of June; and about the 15th symptoms of inflammation appeared, a few days after which he died.

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[*] Sonnini's Bibl. Phys. Econ. Sept. 1809, p. 168.

Dissection of the latter.

Foreseeing this event, I had obtained permission of the owner, to dissect the animal after his death, and dispose of his skin. My first care was to remove this with due caution, that I might be able to preserve the natural shape of the animal in stuffing it: after which I proceeded to examine the viscera, the articulations, and the general disposition of the muscles.

Linea alba.

On opening the abdomen I found the linea alba, or aponeurosis uniting the large muscles, was extremely strong, and much thicker than is usually observed in other quadrupeds.

Abdominal viscera.

The viscera of the abdomen exhibited marks of the inflammation I have mentioned. The stomachs were distended with gas, and the mucous membrane inflamed. The epidermis of the ruminating stomach had already peeled off, and the intestines were nearly in the same state. There was no urine in the bladder. The epiploons exhibited nothing but very thin membranous skins destitute of fat.

Stomachs.

The distribution and figure of the stomachs were the same as in the camel. The second [*le bonnet*] was full of vesicles, from which a serous or aqueous fluid issued abundantly. The paunch and the other two stomachs did not differ in the interior form of their cavities from those of the camel. The cellular stomach [*la poche à cellules*] was remarkable for the internal arrangement of the cells; they having apertures of communication furnished with membranous valves, which no doubt may still be discovered in the dried stomach of the animal. The last stomach is united to a portion of intestine, which may be considered as the duodenum. This was continued in another intestine of equal bulk, which, after forming an arch in the circumference of the abdomen, terminated in the left lumbar region in a *cul-de-sac.*; whence issued another intestinal tube, very slender and smooth, and forming ten or twelve concentric circles in the space made by the former. The circumvolutions were attached to a common mesentery. This slender

Intestines.

intestine afterward made a thousand circumvolutions in the abdomen, terminating at length in another cæcum, without an appendage like the former; whence issued a portion of intestine of considerable bulk, which, after forming two or three curves in the manner of a colon, terminated in the rectum.

Thus it appears, that the vicuna has three sets of intestines, the first and third large, and the middle slender.

No worms in them.

I met with no worms in the intestines, the infinite windings and intersections of which would appear favourable to their formation.

Liver.

The liver, which I did not at first perceive, was found deeply concealed behind the stomach, and attached by very close membranous ligaments to the crura of the diaphragm, and to the corresponding dorsal vertebra. It was of very small bulk, of an oval figure, flattenned transversely, and exhibiting two lobules at its anterior edge. It was destitute of a gall bladder; and the bile was taken directly from the liver by a duct, that conveyed it into a portion of the duodenum. This duct and the vena portæ crossed each other.

No gall bladder.

Spleen.

The spleen, which was likewise very small, and of a rounded form, was situate in the left lumbar region, contiguous to the kidney of the same side. These two organs were enclosed in one common duplicature of the peritoneum.

Lungs.

The lungs exhibited nothing remarkable. They partook of the general inflammation, and the bronchiæ were filled with a frothy sanguineous fluid. The trachea and larynx had the same figure and organization as those of the camel.

Heart.

The heart, which was of a size proportional to the animal, formed almost a perfect cone; only its point, which was very accute, curved upwards and to the left, and the cavity of the ventricle on that side reached to the point.

I did not see the brain, as I wished to preserve the skull entire.

After having examined the viscera of the animal, I proceeded with the dissection. The cartilaginous state of the extremities of the bones did not allow me to make an artificial skeleton of them.

Among the bony parts of the thorax the sternum merits some attention. It is in a horizontal plane, like that of the camel, thick, rounded on its outward surface, and covered in the natural state with a fatty substance of a close texture. The integuments on this part are much thicker than elsewhere. This bone was intended to serve as a point of support for the animal when lying down; and the almost constant use he made of it during his illness had pressed the extremities of the sternocostal cartilages inwards. The middle, spinal apophyses of the vertebræ formed a gibbosity, which, if it had been covered externally by a little fat, would have resembled the bunch of the camel. The remainder of the vertebral column inclined imperceptibly toward the pelvis, which was of itself inclined and of small capacity. The edges of the haunch bones were cartilaginous. The sacrum was lengthened by a series of caudal vertebræ, so as to form a tail in every respect similar to that of the camel.

Hump on the back.

Tail.

The scapulæ, very thin and without clavicles, were connected with the trunk only by means of scapular muscles, as in the camel. The cervical vertebræ formed a very long column, curving from below upward, so as to give the neck the same figure and length in proportion to the size of the animal as those of the camel. As in the latter these vertebræ had no spinal apophyses; but a very strong cervical ligament, extending from the occiput to the spine of the first dorsal vertebra, supplied their place for the attachment of the muscles, and kept the head and vertebræ in their proper position. The anterior face of these vertebræ had a longitudinal hollow, adapted for the reception of the trachea and œsophagus.

Shoulder blades.

Neck.

The head of the vicuna has the same shape and external characters as that of the camel. The jaws have the same number of grinding teeth. The lower has only four cutting teeth, the middlemost of which are the most prominent. The upper has none, as in other ruminating animals.

Head.

The fore and hind limbs in every respect resemble those of the camel.

Limbs.

The joints of the limbs form a perfect ginglymus, admitting a direct and complete flexure of one part against the next, so that this animal, like the camel, bends all his four legs underneath his breast when he lies down: and

Joints.

and this double flexure is the effect of the natural structure of the limbs, as in the camel, which I had an opportunity of studying in Egypt, and of examining from its birth to its adult age. It is not therefore the result of training.

Feet. The feet of the vicuna are terminated by two, long, narrow, soft soles; and have much resemblance to the feet of young camels.

The outward figure of the head perfectly resembles that Ears. of a young camel, except in the ears, which are erect and smooth like those of a kanguroo. The neck, body, and Wool. limbs are similarly disposed; and the body, like it, is covered with a fawncoloured, silky wool, but of extreme fineness. From it may be made stuffs as soft and fine as the shawls of Casimire. This tufted fleece keeps the animal so warm, that it seeks and prefers for its habitation the summits of mountains covered with snow. If the ears of this animal were uniformly cut, it would exactly resemble a camel two or three months old.

Manners. The vicuna has the same cries as the camel, the same gait, and nearly the same disposition. It is extremely shy and timid. It utters plaintive cries at the least unpleasant sensations; and when too much alarmed its eyes are filled with tears. The very active movement of its tail and ears indicate its different sensations. It is very gentle and caressing when tamed.

A little camel. The resemblance the vicuna bears to the camel in its external figure, internal stucture, and qualities, would lead me to call it *camelus parvus auribus rectus*, the little camel with erect ears.

The owner of the animal gave me the following account of the Peruvian mode of hunting it.

Mode of hunting it. The vicunas commonly inhabit the frozen summits of the high mountains of the Cordilleras. Several of the inhabitants assemble together to hunt them. They first surround the mountain where they are most numerous; and by means of mournful cries, or the discordant sound of large wind instruments, as hunting horns, they terrify the animals, who take flight to the summit of the mountain, where no doubt they suppose themselves inaccessible. Here the hunters form a line of circumvallation with stakes, on which are

s all

small red flags. These stakes are connected with each other by cords placed pretty close. Two or three hunters then attack the herd, which disperses. Frequently some of the vicunas are surprised, and the rest rush down the mountain, but as soon as they reach the fence, instead of leaping over it, which they might easily do, terrified at the colour of the flags, they crouch down in the snow, or in holes, where hunters posted for the purpose easily take them. After tying their legs, they carry them to a convenient place, to sheer their fleeces. If the animals be old, they let them loose: if young, they take them to their huts, keep them, and train them to carry burdens, loading them in the same manner as camels. They cannot live in the burning plains of America, and accordingly the inhabitants of the mountains alone can keep them. This no doubt is the reason why the animal has been hitherto so little known.

Domesticated.

When the animal is young, its flesh is good eating; but the wool is justly in high estimation. The merchant assured me, that it was seldom sent to Europe pure, being almost always mixed with other wool of less value.

Flesh and wool.

The latter commonly adulterated.

I think with him, that it might be naturalized and breed in the Pyrenees, on the summit of which the snow scarcely ever thaws; particularly as the pasture there is excellent.

The Pyrenees adapted to it.

X.

Observations on the Hydrosulphate of Soda, and improving the Preparation of the Soda of the Shops: *by Mr.* FIGUIER, *Prof. of Chemistry at Montpellier*[*].

THE 41st vol. of the Ann. de Chim. contains a note by Mr. Vauquelin on hidrosulphuret of soda accidentally found by him in the mother water of a solution, from which he had obtained crystals of carbonate of soda[†]. The soda he lixiviated was from the manufactory of Messrs. Payen and Bourlier; and he supposed, that they had not employed carbonate of lime enough to saturate all the sulphur arising from

Hidrosulpuret of soda found mixed with the carbonate.

Apparent source of it.

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[*] Ann. de Chim. vol. LXIV, p. 59.

[†] See Journal, vol. I, p. 302.

But it exists in the crude soda.

the decomposition of the sulphate of soda by charcoal; and that this was the cause of the formation of the hidrosulphuret. This was probably the case, and no doubt those manufacturers availed themselves of the discovery of Mr. Vauquelin. The following observations however show, that the hidrosulphuret exists also in the soda obtained in the combustion of the plants, that furnish this alkali.

Sulphuretted hidrogen evolved in preparing Rochelle salt.

On saturating a lixivium of Alicant soda with the tartarous oxidule, in order to prepare the tartrite of soda and potash, during the effervescence I perceived a very evident smell of sulphuretted hidrogen gas. Reflecting on this, I imagined the evolution of this gas must be owing to the decomposition of some hidrosulphuret, contained in the alkaline lixivium; and I determined to make some experiments for the purpose of satisfying myself on this head.

A lixivium of soda set to crystillize.

Two sorts of crystals from the mother water.

One hidrosulphuret of soda.

Taking a certain quantity of the lixivium, I evaporated it so as to separate the greater part of the carbonate of soda by crystallization. After it had stood at rest a few days, I decanted the liquor, and put about two quarts into a glass vessel, which I placed on a shelf in my laboratory. After a month I examined it, and found the bottom of the vessel strewed with crystals of a colourless transparent salt, in rectangular tetraedral prisms, terminated by quadrilateral pyramids. I likewise observed octaedral crystals with rhombic bases. The supernatant liquor, being decanted and evaporated, furnished a fresh quantity of the same salt, differing only in being coloured, and in the octaedral crystals being less abundant. The geometrical figures of these crystals led me to presume, that they were a mixture of hidrosulphuret and carbonate of soda, the latter being the smaller quantity. The prismatic crystals, being separated from the others, exhibited the chemical characters of hidrosulphuret of soda. They had an acid and caustic taste, followed by considerable bitterness. They diffused a slight smell of sulphuretted hidrogen gas; and acids poured into a solution of them expelled this gas in some quantity. Not fused, they gave a green colour to blotting paper. With the solutions of sulphate of iron and of copper they threw down a black precipitate, as well as with those of acetate of lead and nitrate of silver. On pouring an acid on the saline crystals in the state in which they had been taken out of the evaporating vessel, a brisk effervescence took place, arising from the extrication of carbonic acid and sulphuretted hidrogen mixed. Willing to satisfy myself whether the formation of hidrosulphuret of soda had not taken place during the boiling of this alkali to extract the carbonate, I took 3gr. [46grs] of powdered soda, put them into a phial, and poured on them muriatic acid. This produced a brisk effervescence, and a very strong smell of sulphuretted hidrogen gas. On passing this gas, by means of a tube, through several solutions of metallic salts; the oxides were precipitated of the same colour as they would have been by sulphuretted hidrogen gas obtained from the decomposition of sulphuret of iron by sulphuric acid.

This not formed during the boiling.

Sulphuretted hidrogen evolved from various specimens.

From a mixture of crystallized tartarous acid in powder and soda sulphuretted hidrogen gas was equally evolved. These experiments were made with the various sorts sold in the shops under the names of Carthagena soda, kelp, and barilla. They all presented the same results.

Crude soda always contaminated with hidrosulphuret.

Source of it.

It cannot be doubted therefore, that the hidrosulphuret of soda is contained in all the kinds of soda; and that it may be obtained from the mother waters of the lixiviums, that have furnished carbonate of soda. The formation of this salt is easily understood. When the plant is burned for procuring the alkali, the fire is urged so far as to cause the ashes to undergo a semivitrification: the sulphates contained in them are decomposed by the action of the charcoal: the sulphur is liberated and forms sulphurets. At the same time there is an extrication of hidrogen gas, which may be furnished by the charcoal itself, or by the decomposition of the plant or of water; and no doubt by the three together. This hidrogen gas, uniting with the sulphur, constitutes the sulphuretted hidrogen gas, which in its turn combines with a part of the alkali, and forms hidrosulphuret of soda.

The preparation therefore defective.

Inconveniences of it.

The formation of the sulphurets and hidrosulphurets that barilla contains being occasioned by the strong calcination of the ashes furnishing this alkali, we may infer, that the method of preparing it is defective. It is evident, that part of the alkaline salt enters into the constitution of the sulphurets and hidrosulphurets contained in it. These remain in solution in the mother waters, that have furnished carbonate of soda. When the barilla is employed for making soap, the same loss is experienced. The soapboiler's lie contains sulphurets and hidrosulphurets, which diminish its causticity, as they have a weaker affinity for lime than for soda. The consumption of this alkaline substance in pharmacy, and more especially in soap-making, is sufficiently extensive, to turn the attention of chemists and manufacturers to an improvement in preparing this alkali; which is also much employed in dyeing cotton, and in the washhouse. In the latter, the sulphurets and hidrosulphurets in barilla not only render it so much the dearer, but are injurious to the whiteness of the linen and cotton*.

Farther inconveniences.

Method of avoiding these.

These are not the only incouveniences arising from the high degree of heat, to which the soda is exposed in manufacturing it. A still greater is, that a part of the alkali enters into combination with the earthy substance contained in the ashes, and forms a kind of frit, indecomposable by the action either of water or of acids; and the quantity of alkali wasted in forming this semivitrified substance is greater than that taken up in the formation of the sulphurets and hidrosulphurets. Here no doubt is a great loss of alkali, occasioned by the semivitrification of the ashes of the plants that furnish soda; and which would be avoided by adopting the mode used in preparing potash; lixiviating the ashes, evaporating the lixivium to dryness, and selling the alkaline salt in this state of preparation. The consumer would find so much

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Serious accident in a soap manufactory.

[*] The sulphurets contained in unprepared soda frequently occasion serious accidents in soap manufactories. In these, where it is customary to keep caustic lie in large covered stone vats, the sulphurets decompose the water. The hidrogen not absorbed by the hidroguretted sulphurets thus formed occupies the empty part of the vat. When the workman takes off the cover, to dip out the lie, and holds in his hand a lamp to light the inside of the vat, the hidrogen takes fire, and endangers the building. In a visit I just paid to Marseilles I saw one of these manufactories, that had been destroyed by a violent explosion of this kind. The hidrogen being mixed with atmospheric air such an explosion took place, that the house was near being thrown down. The owner, supposing the manufacturer had maliciously attempted to destroy his house, summoned him before the magistrate; and the cause is still pending before the first tribunal of the department.

the more advantage in this, as it would be easy for him to satisfy himself of the purity of the alkali; a knowledge so necessary to the success of various processes both in pharmacy and in the arts.

Reflections on the manufacture.

But what are the reasons, that have induced the manufacturers of barilla to give it this solid consistency? Is it because in this state it is more convenient for carriage? or because it was originally less used in the making of soap than of glass? It is true, that in the glasshouse there are fewer inconveniences from its use, than in the processes of chemistry and the other arts. When the crude alkali is employed in making glass, not only are the hydrosulphurets, sulphurets, and other salts it contains, decomposed by the high heat required in this process, and their alkali serves as a flux; but the frit itself enters into the state of vitrification, and thus adds to the bulk of the glass. This, however, cannot be considered as an advantage; for it is certain, that barilla contains nearly four fifths of its weight of heterogeneous substances, which of course increase the expense of carriage in this proportion. This is a consideration, that claims the attention of the consumer. The first source of profit in a manufactory is economy in the raw materials.

Hint to manufacturers.

XI.

An Essay on the Cultivation of the Red Beet, by Mr. GOERING, *a Saxon Agriculturist*[*].

Red beet very beneficial.

Yields much good sugar,

but this consumes too much wood.

NEXT to the potato, the utility of which is well known, the red beet is one of the most beneficial plants, the cultivation of which is particularly to be recommended. Every one knows, that sugar has been obtained from it not inferior to that of India; and the manufacture of which would probably have been established in Germany, had not the consumption of wood necessary for it checked its most zealous partisans; for the resources of Germany in this respect are daily diminishing.

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[*] Sonnini's Bib. Phys. Econ. May, 1810, p. 289.

Beside

Other properties of it.

Beside this essential point, which cannot be attained from local difficulties, and which may not exist in many other countries, the principal properties of the beet are those of being nourishing, emollient, cooling, laxative, &c.

Advantageous for feeding cattle.

White beet injures the milk:

Supposing it to be cultivated only for feeding stock, particularly cows, in winter and in summer, it deserves in every respect to be preferred to most plants both for the root and leaf. Though the white beet is of pretty extensive use, and much cultivated, it cannot in any respect be compared with the red. It is neither so firm nor so sweet; and we find by experience, that the milk of cows fed some time with it loses its sweetness, and becomes bitter. Besides, it can scarcely be kept through the winter, as it soon grows rotten.

the red beet improves it.

The red beet on the contrary is firm, sweet, and but in a moderate degree watery. It is at least as nutritious as the turnip cabbage, and imparts to the milk a pleasing sweetness, which continues as long as the cow is fed on it. It keeps very well through the winter, either in cellars or in pits, provided it be not put in wet; and is as fresh when taken out in the spring as it was when laid up. They who cultivate both sorts, therefore, should use the white in the fall, and keep the red for the spring.

The leaves good fodder.

The leaves of the red beet, which may be gathered in the middle of july, the time of sowing the white beet only, is excellent fodder, particularly for horned cattle and pigs. It is true however, that the leaves cannot be thus gathered but at the expense of the roots.

A crop to be depended on.

It is also indisputable, that the red beet is one of the roots that succeed almost always. It has few enemies, and a good crop may always be depended on, provided the ground has been well tilled and prepared, and the seed properly sown.

May be sown at any time,

and no insect injures it.

There is no season amiss for sowing the red beet. It may be sown as early as you please in spring, or even in autumn; for the first leaves which in most other plants are very tender, are able to stand the cold winds of spring. No insect can hurt them; and while the turnip, the turnip cabbage, the cabbage, &c., are destroyed by the leaflice, the red beet grows astonishingly: and when in autumn the leaves of those plants are devoured by caterpillars, none are seen on the red beet.

Fowls extremely fond of the leaves.

The only enemies it has, that I know of, are fowls; for these are so fond of its leaves, as entirely to lay waste the fields of it, to which they can have access. Their appetite for this plant, when they once have discovered it in a field or garden, is such, that it is almost impossible to keep them out. They should not be sown therefore in gardens or fields too near houses, as in this case the crop may be looked upon as lost.

The following is the method I have adopted of cultivating it.

Method of cultivating it.

I first select, if possible, a good black mould, rather rich. If it be mixed with a little sand, and provided it has not too much clay, it is good for the beet, which always requires a little moisture. It may be cultivated indeed on light ground, but not with equal success.

In autumn I lay on manure, in the proportion of six two horse cart loads of dung of horned cattle to a hundred and forty square perches. This dung I afterward bury at least six inches deep with the plough: and then I give the ground another ploughing in narrow furrows.

As soon in the spring as the land can be worked, I sow the eed where the plants are to remain; for experience has taught me, that transplanting them is injurious. They should not be sown too thick: there should be at least six inches distance between the plants; and it is often necessary to pull up some in the thickest places, for three or four plants frequently spring from a single seed.

It is usual to cover the seed by raking or harrowing: but as from their lightness they frequently lie on the surface and rot, it is better to use the hoe, or the plough, taking care not to bury them too deep. In this way we may be certain of their germinating quickly, if the soil be good.

As soon as the plants have their sixth leaf, they should be weeded, and thinned out where too close. A few weeks after they should be hoed, but so as rather to draw the earth from them than to heap it round them.

Gathering the leaves.

When the leaves begin to bend down to the ground, the largest, at the bottom of the plant, may be gathered for the cattle: but they must not be stripped too much, as this would injure the root. Nor should the leaves be plucked off before they separate as it were of themselves, inclining toward the ground. If

Taking up and preparing the roots.

If weeds appear again, or the ground get hard and dry, they should be hoed a second time. Lastly, in the month of October the roots should be taken up, and laid in the places intended for keeping them, first cutting off the stalk close to the root, that they may not vegetate during the winter.

XII.

Account of a Composition commonly called Turkish Rose Pearls; by Mr. MARCEL DE SERRES, *Inspector of Arts, at Vienna*[*].

Elegant and fragrant black beads made of rose leaves.

TURKEY has a considerable trade in a composition known by the name of rose pearls; and as this composition is very simple, I imagine it may not be uninteresting to make it known, that it may be imitated in other countries. Nothing more is necessary than to take the petals of fresh gathered roses, and pound them carefully in a cast iron mortar well polished. They are to be pounded till they are thoroughly bruised and form a smooth paste. This paste is to be spread on a sheet of iron, and dried in the air. When it is nearly dry, it is to be pounded again with some rose water, and dried afresh. This is to be repeated, till the mass is reduced to a very fine paste, when it is fashioned into the proper shape with the fingers, or with an instrument similar to that used for cutting pills. The sort of beads thus formed are then perforated for stringing, and the paste is dried afresh, till it becomes very hard. When they are smooth and well polished, they are rubbed with oil of roses, to increase their fragrance and lustre. By this simple process the paste of rose leaves takes a very decided black colour, owing to a combination of the gallic acid in them with iron.

Similar beads of other colours.

With a similar paste beads of various colours are formed. The most common, next to the black, are red and blue. The colouring matter is added to the paste. It is possible however, that these red or blue beads, which are said to be nothing but the paste of rose leaves so coloured, may be made of a particular paste; and if I must give my opinion, I should think this is the case, from the difficulty of giving a red or blue colour to a paste so black as that of roses*. The red necklaces in question must not be confounded with those made of pimento, or those of the fruit of the red bead vine, *abrus precatorius.*

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[*] Sonnini's Biblioth. Phys.—Econ. Feb. 1810, p. 105.

Additions to their perfume.

Frequently to render the Turkish rose pearls more fragrant, oil of roses, storax, and musk, are mixed with the paste; but this addition makes no alteration in the mode of preparing it.

The black generally preferred.

The black beads are most prized, either because they set off the colour of the skin to more advantage, or because their perfume is more agreeable. These beads find their way over Europe through Austria, and are of some consequence as an article of trade.

XIII.

On the tall Oatgrass: *by Mr.* Tollard, *sen*†.

Utility of the tall oat-grass.

THE tall oatgrass, *avena elatior,* grows and produces an abundance of fodder, both in good and bad soils. It is of very early growth, and rises to the height of two or three feet. Its stalk is fine, slender, and makes very good hay. It is mowed twice a year. If it be eaten green, it may be cut oftener; but it is principally cut for hay.

It may be sowed in autumn, or in spring, after two ploughings; at the rate of 70 kil. [154 lbs] to the *are* [2·5 acres]. Frequently saintfoin is sown with it, in the proportion of a hectolitre [2 bush. 3 pecks] of saintfoin, and 60 kil. [132 lbs] of oatgrass seed, to the above quantity of ground.

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[*] It is obvious, that this difficulty would be removed by wholly avoiding the use of iron in making them. At the same time the petals of other fragrant flowers of different colours might probably be used with advantage. Thus the violet appears to be well adapted for the blue The manufacturer too might avail himself of the well known property acids possess of heightening the red of roses, and of changing vegetable blues to red; as well as perhaps that of alkalis, in converting the blues into green. C.

[†] Abridged from Sonnini's Journal, Dec. 1810, p. 375.

Utility of the tall oatgrass.

It is particularly adapted to horses; but all animals, that are commonly fed with hay, eat it with pleasure.

Opinions have been so divided respecting this plant, that several writers have been eager to boast its advantages, while others have endeavoured to depreciate it. This difference of opinion respecting a plant of real utility has arisen from the authors who have mentioned it omitting its botanic name: hence some have confounded with it the ray grass, *lolium perenne*; others with the way bennet, *hordeum murale*, which has no relation to it, and is one of the very numerous plants injurious to meadows.

I repeat, that the *avena elatior* is the best basis of a natural meadow; and that, when cultivated alone, it makes an excellent pasture. It is one of the best of the family of grasses, as any one may readily be convinced by observation. It may be known any where by its slender stalk, rising above the other grasses, and terminating in panicles a little drooping.

SCIENTIFIC NEWS.

St. Thomas's and Guys' Hospitals.

Medical and surgical lectures:

The Winter Course of Lectures, at these adjoining Hospitals will commence as usual on the 1st of October, viz,

At St. Thomas's.

Anatomy and the operations of surgery, by Mr. A. Cooper, and Mr. Henry Cline. Principles and practice of surgery, by Mr. A. Cooper.

At Guy's.

Practice of medicine, by Dr. Babington and Dr. Curry. Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen. Experimental philosophy, by Mr. Allen. Theory of medicine, and materia medica, by Dr. Curry and Dr. Cholmeley. Midwifery and diseases of women and children, by Dr. Haighton. Physiology, or laws of the animal œconomy, by Dr. Haighton. Structure and diseases of the teeth, by Mr. Fox.

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and, with the lectures on anatomy, and those on the principles and practice of surgery, given at the Theatre of St. Thomas's Hospital adjoining, the whole is calculated to form a complete course of medical and chirurgical instructions.

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OCTOBER, 1812.

ARTICLE I.

On the Electric Column, and Aerial Electroscope. By J. A. De Luc, *Esq. F. R. S.*

To William Nicholson, Esq.

Sir,

1. In my last paper, published in your No. 149, after having shown, that Dr. Maycock had very ably refuted the natural philosophers, who thought that the *galvanic effects* depended on the *electrical energies* of the particles of *matter*; and proved, that it was produced by the action, on each other, of two proper *metals*; I was obliged to dissent from him on two points: 1. that the *electrical excitation* produced by the two metals did not exist during their *contact*, but only at the instant they were *separated*—2. that the *galvanic apparatus* can only be *excited* by a *decomposable fluid*, which is always *decomposed* when the apparatus acts. But I opposed to these two propositions my experiments related in your Journal for June and August, 1810, which demonstrate, that, in the *galvanic pile* the *chemical* effects are independent of the cause of the *electric* effects; the former being only produced when the *electric fluid* pervades a *pile*, in which a *liquid* acts on the *metals* to *corrode them*. I have explained, also, in the same paper, how these

Two points on which the author dissents from Dr. Maycock.

His reasons for this.

experiments led me to the discovery of an *electric apparatus*, which, without any *liquid*, and composed only of alternate *zinc plates*, and equal pieces of *Dutch gilt paper*, produces strong *electric*, but no *chemical* effects.

Spontaneous electric machine

2. My paper, Sir, in your Journal for October, 1810*, contains the description of a small apparatus of this kind, which, in order to distinguish it from the *galvanic pile*, I have named *electric column*. It is really a *spontaneous electric machine*, the power of which can be increased without limits, by increasing the number of the groups: it is lasting; for I have still the first *column*, (that represented in the *figure*†) which I constructed five years ago, preserving still its power: and indeed there is no reason why it should lose it, as there is no *liquid* to affect the *metals*; for it requires only that the *papers* should possess the small degree of *moisture* of the surrounding air.

of unlimited power and duration.

Column of 20000 pieces, by Mr. Allen.

3. As to the increase of power of this natural *electric machine* by increasing the *number* of the *groups*, a very ingenious and well-known experimental philosopher, Mr. W. Allen, has carried it to such a degree as to produce very remarkable effects. His apparatus consists of ten *columns*, each containing 1000 groups of *zinc* and *Dutch-gilt paper*, forming together a series of 10,000 *groups*: but they are of a *small* size, so as to be enclosed in glass tubes; a circumstance, the effect of which shall be seen. The following are the phænomena observed by Mr. Allen, which confirm some points which I have stated.

Jar charged by it.

1. Having tried to *charge* a *coated jar*, the *charge* arrived sometimes to such a degree, as to give a *shock* up to the elbows; but at other times it could never arrive at that point. This circumstance is owing to what I had observed of the influence of the *electrical state* of the *air*.

Did not decompose water.

2. Notwithstanding such *electric* power, Mr. Allen did not perceive any production of *gasses* in *glass tubes with water*, made to connect the extremities of these *columns*.

Effect of size and number of plates.

3. That apparatus shows also what I had found with respect to the *size* of the *plates*; that the *size* was indifferent to the *intensity* of the *ultimate effect*; but that the *larger* they were, the *sooner* that *effect* was produced. The *plates* of Mr. Allen being *small*, many minutes were required to *charge* his *jar*.

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* Vol. xxvii, p. 81.

† Ib. pl. iii.

Mr. De Luc's apparatus.

4. I come now, Sir, to my experiments contained in your Journal for October, 1810, with a *figure*, half the size of the original, of the apparatus to which I shall refer them, but only in their parts concerning Dr. Maycock's system. For these experiments, beside the *electroscopes* at the *ends* of the *column*, there is one in the *middle*. I shall call A, that connected with the *positive* end; B, that at the *negative*; and C, an electroscope placed at the *middle point*: but by taking off the wire 4, it may be made to communicate with any part of the *column*, the *electric* state of which is wanted to be known; and this by means of an insulated *wire*, so soft as to be easily bent, and thus made to connect, by one end, with the electroscope C, and by the other, with the part of the *column* the *electrical* state of which is wanted to be known.

5. These experiments, which begin at p. 88 of the same number of your Journal, referring to some TABLES given in the conclusion of my preceding paper*, I think better, for an immediate reference, to copy them here, with this previous explanation, that in all of them, A indicates the *positive* end, and B, the *negative*.

Different states of the pile.

TABLE I. *Insulated Column.*	TABLE II. *B in communication with the ground.*	TABLE III. *A in communication with the ground.*
A	A	A
+ 10	+ 20	0
+ 8	+ 18	— 2
+ 6	+ 16	— 4
+ 4	+ 14	— 6
+ 2	+ 12	— 8
0	+ 10	— 10
— 2	+ 8	— 12
— 4	+ 6	— 14
— 6	+ 4	— 16
— 8	+ 2	— 18
— 10	0	— 20
B	B	B

Experiments showing the motion of the electric fluid in the column.

6. I come now to the experiments. Experiment 7 is thus related in p. 88 of your Journal above quoted. “ At the time “ when (on account of the *electrical state* of the *air*) there are “ simple and equal *divergences* in the electroscopes at the extre-

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* Vol. xxvi, p. 265.

"mities of the *column*, then *positive* at A, and *negative* at B; "there is no *divergence* in the electroscope C; it is a *neutral* "point, and is expressed by 0 in TABLE I. If, at the same "time, any point of the *column*, at a distance from the point "C, on the *negative* or *positive* side, proportional to one of "the terms of TABLE I, be tried by the insulated wire, the *di*"*vergence* produced is, as exactly as can be expected in such "experiments, correspondent to that expressed in the *table*, with its sign either — or +."

7. In *Exp.* 8 are seen the two different cases expressed in TABLES II and III, observed also by means of the *insulated wire*. The case of TABLE II is produced by placing the end B, or *negative*, in communication with the *ground*. In this case, the electrometer at the *middle* point C, which, in the preceding case, was *neutral*, has a *positive* divergence, equal to that before observed at the extremity A, where it is now *double*. There is no *neutral* point perceptible, but close to the end B, whence the *positive* state is increasing towards A, at the rate expressed by TABLE II. If the communication with the *ground* be inversely placed at A, or the *positive* extremity, which is the case of TABLE III; then the only point found *neutral* in the *column* is close to that end A; and thence the *negative* state is increasing up to B, where it is become nearly *double*; and at the middle point C, the divergence, now become *negative*, is equal to what it is at the extremity B, when the *column* is *insulated*.

The effects produced while the metals are in contact.

8. If Dr. Maycock had known these experiments, in which the electroscopes indicate, to the eye, the *motions* of the *electric fluid* in the *column*, he could not have persisted in his opinion, that the *metals* do not produce their *electrical* effects while *in contact*, and only at the instant they are *separated*; for they were not *separated* an instant during the course of these experiments. Besides, I am induced to think, that Dr. Maycock did not even know yet the existence of that new instrument, the *electric column*; or at least had not had the opportunity of seeing any; for each *column*, of whatever *number* of *groups* it is composed, which constantly remain *in contact*, produces *electrical* effects at its extremities, in proportion to the *number* of *groups*.

This equally takes place

9. Dr. Maycock, not having read my papers, might say, that he had applied his system only to the *galvanic pile*; but had he known

in the galvanic pile.

known the same paper of which I am speaking, he would have seen, in p. 91, that I have made the very same experiments directly on the *galvanic pile*; and that I found the same gradation of *plus* and of *minus*, from the *middle point*, as in the *column*. Only these operations cannot be so long continued on the *pile*, because its *electrical signs* are diminishing in proportion to the *erosion* of the surface of the *metals*. But Dr. Maycock could not be informed of these *electrical* phænomena of the *galvanic pile*; because, as it appears, he has not used it; and his galvanic observations have been only on the galvanic apparatus of *troughs*, which, as I shall show in a future paper, has deceived him.

Troughs act differently.

Other proofs of the circulation of the electric fluid in the pile.

10. I now come to another class of experiments, which, it seems, are also unknown to Dr. Maycock; for, had he read them, he would have found evident proofs of a *irculation* of the *electric fluid*, when the extremities of the *column*, or of the *pile*, are *connected* together. These experiments consist in producing the *connexion* of the *extremities* by different *bodies*, and observing their effects on the *gold leaf* electroscopes. It has been seen, in the above experiments, that, when the extremities of the *column* are *unconnected*, there is an *accumulation* of the *electric fluid* at the extremity A, where the *gold leaves* diverge as *positive*, and a *deficiency* at the extremity B, where they diverge *negatively*. If a good *conductor* be applied to produce the communication between the extremities, the gold leaves *fall* on both sides: if it be a perfect *nonconductor*, their *divergence* is not altered: but if an *imperfect conductor* be applied, they *fall*, in proportion to the *conducting* faculty of the body.

Glass requires varnish for a perfect insulation.

11. These experiments begin at p. 91 of the same number of your Journal. I made them for the purpose of ascertaining a very essential point in electricity, that of the best *insulation* of all our *electrical apparatuses*; having found, that the want of a complete *insulation* may lead to errour. *Glass* is the only body used, on account of its solidity, for pillars in all these apparatuses; and it has, in this respect, the essential property not to be *permeable* to the *electric fluid*: but it is not a perfect *nonconductor*; the *electric fluid* moves, though slowly, along its surface, and to prevent it, it is necessary to cover it with some *insulating varnish*. These experiments, therefore, I made first, in order to find out which were the *best conductors*;

next, what was the best *insulating varnish* to cover *glass*; and in their course I ascertained the different *conducting faculty* of various bodies.

Conductors and nonconductors.

12. The general results of these experiments were, that with respect to the *best conductors*, the *glass tubes* with *water* and proper wires, when no *chemical* effect is produced in them, are sensibly as good conductors as *metals*. As to the *insulating* faculty, I found, that *sealing wax*, in which no spirit of wine is added to make it softer, being laid on *glass rods* sufficiently heated to melt it, is equal to the best other *varnish*; for when placed on the extremities of the *column*, with the precautions I have indicated (the want of which produce very remarkable phænomena) these rods do not affect the *divergence* of the *gold leaves*. Lastly, I have given many details of my experiments on intermediate bodies, showing that, in proportion to their *conducting faculty*, each produces a determined degree of *diminution* in the *divergence* of the *gold leaves*.

The experiments prove a circulation of the fluid.

13. The whole of these experiments affords such proofs of a *circulation* of the *electric fluid* in the *column* when its extremities are connected together, and consequently of its *motion*, that, if Dr. Maycock had known them, he could not have had any doubt of these effects. The *circulation* is in consequence of an *accumulation* constantly tending to be produced on the *positive* extremity, at the expense of the other. This tendency continues, though the extremities are connected together; but the *electric fluid* cannot *accumulate* on the *positive*, while a *good conductor* can transmit it instantly to the *negative*; whence it also instantly returns to the *positive*, by the property of the column. But if the intermediate body be an *imperfect conductor*, the *circulation* is lessened, and some *electrical* signs remain at the extremities.

The electrical action not sudden.

14. There is another set of my experiments, which might have made Dr. Maycock doubt of the very ground of his system. He has imagined a certain *property* producing the *electrical* effects; such, I suppose, as that of the *magnet*; which, in consequence, ought to act *suddenly*. If this were the case, when a communication with the *ground* has changed the *divergences* of the *gold leaves*, that communication being removed, the same *divergences* ought to be *suddenly* restored; but it is far otherwise, as may be seen in Exp. 4, 5, 6, of the same paper.

There is some sort of impediment to the *motion* of the *electric fluid* along the *column*, probably caused by a *reluctance* in the *zinc plates* to part with the superior quantity of *electric fluid* they must possess when united with *copper*, to produce their *electrical equilibrium*. The consequence is: that, after having observed the *divergence* of the *gold leaves* in both electrometers, if one of the extremities of the *column* be made to communicate with the *ground*; by which the gold leaves *fall* on this side, and they *diverge* more on the other side; it requires a long time, in some cases many hours, for the same *divergences* to be restored.

But progressive.

15. There is an entertaining experiment, which may lead to some discovery respecting the physiology of *vegetables*. Each of the *electrometers* of the *column* may be made to imitate the *sensitive plant (mimosa sensitiva:)* for, as the *contact* of one of the extremities of the *column* produces the *fall* of the *gold leaves* on this side, which rise *slowly*; the contact of the *sensitive plant* makes its leaves *fall*, and they also rise *slowly*. This analogy of *slow* effects, pointing out some general analogy between their causes, must render us cautious not to assign hastily to some vague *property* the effects that we may follow distinctly in their process, such as those of the *electric column*; for they may lead us, in time, to the discovery of causes, in those phænomena which now appear the most obscure.

Similarity between the action of the column, and that of the sensitive plant.

16. The IId part, Sir, of the same paper in your journal, concerns the *electric column* in its phænomena as an *aerial electroscope*, and contains the observations which I had already made with that instrument. This class of experiments relates to the opinion of Dr. Maycock, forasmuch as they prove, not only a constant *motion* of the *electric fluid* in the *column*, but that some external cause influences much the *rapidity* of its *motions*; an object the explanation of which I had postponed.

The rapidity of elective circulation influenced by some external cause.

17. These changes are seen, when the *extremities* of the *column* are not made to communicate immediately with each other; but only by the alternate *strikings* of a body suspended between them, taking some *electric fluid* from the *positive* side, and bringing it back to the *negative*. Now, the more *rapid* is the *motion* of the *electric fluid* in the *column*, the more *numerous* are the *strikings* in a *given time*; and the difference is

As shown by the aerial electroscope.

is very considerable, in different days, and different parts of the same day.

This instrument made known imperfectly,

18. When I first observed that phænomenon, it pointed out to me a new and very interesting object of study: but, according to a plan of observations which I then formed, I was obliged to make many additions to my *column*, which required much time: but the first description which I had given of that apparatus in a paper to the Royal Society, and of its purpose, made it partly known.

and imitated by Mr. Forster.

19. This accidental communication to the public was a lucky circumstance; for before I could have time to do it myself through your Journal, a very ingenious experimental philosopher, Mr. *B. M. Forster*, not knowing it precisely, imitated it in a curious manner: he formed two *columns*, containing together 1500 groups of *zinc* and *silvered* paper, of the small size of my first *column*, and having placed them horizontally, he connected with each extremity a small *bell*, and suspended between them, and very near them, a small brass *ball*, held by a silk thread. When the apparatus was ready, he heard it *chime*, with a sort of buzzing noise on account of the rapidity of the motion of the ball.

Defect of his apparatus.

20. This apparatus had been mentioned in Mr. Tilloch's Phil. Magazine, and having seen there its description, I spoke of it in the same paper of your Journal, p. 103. But since that time, having had the pleasure of making personal acquaintance with Mr. Forster, and corresponding with him, he has communicated to me, from time to time, his observations of this kind of *aeroscope*, which, though in a different manner, indicates also changes in the *electrical state* of the *air:* for, after having chimed for some time, it stops totally, then begins again, and stops; sometimes it chimes for a moment, between long intervals of silence. This is a very curious phænomenon, but there is a want of intermediary *terms* between the *cessation* and *return* of motion. These inequalities are occasioned by the insulation of the little *ball*, it being suspended by a silk thread. Having tried what would be the effect of a greater distance between the bells, I found that it stopped the motion of the little ball, and I soon judged what was the cause of that cessation. When there is more distance, the little ball tending sensibly as much to the *positive* as to the *negative* bell, the dif-

ference between these tendencies is not sufficient to surmount its weight, and it remains without motion; but when it is very near each bell, a very small difference of attraction on one side can make it move towards it, whence it is repulsed. The difference, however, between these attractions may be so small, that the little ball remains undetermined, even at that small distance, though the *column* has a sensible action.

This did not exist in Mr. de Luc's.

21. My plan had been different from the beginning, and thus free from that impediment: it was to obtain a separate *electrometer*, formed of a long brass rod with a large ball at the bottom, and to suspend at the top, by a conducting thread, a small metallic ball. This small apparatus being connected by its upper part with one side of the *column*, the little ball was to diverge; and I intended to have another large ball in communication either with the other side of the *column*, or with the ground, against which the little pendulum should strike, fall, and rise again. This apparatus is represented in the figure annexed to my paper in your Journal for October, 1810. In the same paper, I explained all the difficulties which I encountered, before I could prevent the little ball of the pendulum from sticking to the large ball. At last, however, I succeeded by the means expressed in the figure; and having determined the distance of the second large *ball*, at which the pendulum should never cease to *strike* it by the smallest power of the *column*, the purpose of the apparatus became to *count* the *number* of the *strikings* in a *given time*; which was the precise indication that I had desired to obtain of the smallest changes happening in the power of the *column*.

Observations with this apparatus.

22. This apparatus was ready for observation in the beginning of April, 1810, and in the remaining part of the same paper I related the phænomena, which it exhibited during this month and the following month of May. The tables of these observations are composed of five columns: the first indicates the *days* and *parts of the days* in which the observations were made. The second, the points at which the *barometer* stood. The third, the points of the *thermometer* in the room. The fourth that of my *hygrometer**. The fifth, the *number* of *strikings* of the *pendulum* in determined *times*. 23. By

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* This instrument has been taken up by a very ingenious Hanoverian gentleman

The phenomena depended on the electric state of the atmosphere only.

23. By comparing the last column with all the others, in the paper above-mentioned, it may be seen, that there is no connexion of the *number* of *strikings* with either the *barometer*, the *thermometer*, or the *hygrometer*, and only with the different *days* and *parts* of the *day*. Which circumstance confirmed me in the idea, that it was only the different *electrical* states of the surrounding *air*, that produced these changes in the power of the *column*; however obscure was still this connexion, for the reasons which I explained.

The inquiry pursued by Mr. T. Forster.

24. This is a new and very interesting subject of experimental and even natural philosophy, and in publishing it in this its infancy, I had the hope that it might lead some attentive observer to follow it up. This hope has been realized, when I have seen in your Journal, that Mr. T. Forster has undertaken that investigation; particularly as I know his talents, being, since that time, personally acquainted with him.

Improvement in the apparatus.

25. I shall only mention farther, that I have made a new step in this pursuit. Knowing by my former experiments, that, though the *size* of the *plates* is indifferent to the final simple *divergence* of the *gold leaf electrometers* at the extremities of the *column*, it is not the same when, one of them *striking* the side, they are reduced to the electrical state of the ground; for they *rise* faster and *strike* again, when the *plates* are *larger*. Applying, therefore, this result of my former experiments to the motion of a *pendulum*, I have constructed a *column*, which, in two connected parts, contains 1300 groups, formed of *zinc* plates 1½ inch square, and equal pieces of *Dutch gilt paper*. This *column* moves a *pendulum* consisting of a *gilt pith ball* the size of a pea, suspended like the other by a *conducting* thread, and placed in the same apparatus, which prevents its sticking when it strikes the large ball. This *pendulum*, guarded against the agitation of the air by a glass case, moves between the two same large *balls*, being near one inch distant from each other, and it has not ceased to strike

gentleman, residing at present at Cumberland Lodge, near Windsor; Mr. Hausemann, he has succeeded in every point, and is resolved, from its utility, to construct it for the experimental philosophers who shall desire it.

during already two years that it has been constructed: but the *frequency* of its *strikings* is also very various; for I have observed at times forty-five in a minute; but passing at other times by all the intermediate numbers down to hardly one.

Mr. B. M. Forster constructing a similar apparatus.

26. In this state I must leave this pursuit, on account of my age; but I have learnt, with great pleasure, that Mr. B. M. Forster is employed in constructing also a *column* with *large plates* and a *pendulum*; and that his nephew, Mr. T. Forster takes great notice of the connexions of this phænomena with various circumstances in the appearances of the air, and with diseases. This, in time, may lead to some useful discovery, both for science and for society.

Mr. Hausemann also observing with one.

27. This new *electrical* phænomenon, so connected with the state of the *air* which surrounds us, cannot but interest many natural philosophers, were it only with respect to meteorology: it is a new *thread* leading in the maze of *atmospheric* phænomena, provided it is not associated with gratuitous hypotheses. This, Sir, has been the object of the IIId part of my paper on the *electric column*, contained in your No. 124, for December, 1810; in which part I have given an abstract of some other *threads* obtained in the *atmospheric* phænomena, considered both in themselves, and in their relation with those exhibited by the spontaneous appearances and disappearances of the *electric fluid*; especially in the great phænomenon of *lightning* and *thunder*. Mr. Hausemann, of whom I have spoken above in a note, having had the opportunity of observing the different *motions* of my *pendulum*, and persuaded that they must have some connection with the *atmospheric* phænomena, has constructed the same apparatus with large plates, and begun regular observations.

28. I stop here on this interesting subject, having, I think, recalled it sufficiently to show, that Dr. Maycock had not embraced, or considered with attention, all the branches of experimental philosophy connected with the determination of the *nature* and *functions* of a *fluid* influencing almost all the atmospheric phænomena. But in a future paper I shall treat of another part of the same subject, by coming to the idea Dr. Maycock has conceived of the effect of *friction*, to produce

electrical effects; which will give me an opportunity of examining his system under a different point of view.

I have the honour to be,
Sir,
Your obedient, humble Servant,
J. A. DE LUC.

Windsor, August the 27th, 1812.

II.

Effect of the Attraction between the Weights and the Pendulum on the going of Clocks. In a Letter from Mr. THOMAS REID.

To W. Nicholson, Esq.

Edinburgh, 18*th Aug*. 1812.

SIR,

The going of a time-piece affected by attraction.

OF late, having been much engaged with astronomical clocks, which require a great deal of attention, to see that they are fit to perform, and to keep as near to time as it is possible from the nature of things to bring them; it will perhaps be thought strange to say, that attraction comes in for a share in those obstacles, which stand in the way of good timekeeping. This is what has never been even hinted at before; if it has, I confess it is new to me. We have heard of clock pendulums disturbing one another, where clocks were set agoing on the same board, and where the pendulums were not sufficiently fixed, but this arose from a very different cause.

A six-weeks astronomical clock.

Having fitted up a clock in every respect particularly good, and unexceptionable both in the plan and the execution of it, which, by express order, was made to go about one month or six weeks, the scapement of it made after the principle suggested by Mudge as far back as the year 1763, and which he afterwards introduced or used in his timekeepers. This clock, from the nature of the scapement, and from that of its pivots being so independent of oil, at almost all of the holes, was, from these circumstances, expected to keep the arc of the vibration of the pendulum as nearly constant as possible; but after keeping this arc perfectly for above two weeks, it surprised me, and even mortified me not a little, to find no constancy in the arc even here, notwithstanding very sanguine hopes had been entertained of it. One morning it was observed to have come in a little, which afterward it did gradually more and more for some time, from a maximum to a minimum, and vice versa, until it had regained its original extent of arc, which it kept on, till the same circumstances came again in the way to disturb it.

Began to diminish its arc of vibration in three weeks,

and after a time increased it again.

Although I saw this, and was in some degree convinced, that this must be owing to the attraction of the weight for the pendulum; yet I would not rest altogether satisfied without again examining the clock, lest something might be there, which tended to give rise to this inequality of the arc of the pendulum's vibration. But on examination, there was evidently no cause, that could in the smallest degree be suspected, for this.

This presumed to arise from the weight attracting the pendulum.

In constructing the clock, attraction was suggested by a very ingenious friend, on an idea taken from the experiments that had been made by Mr. Cavendish: and accordingly the barrel was so contrived, as to throw the weight, when it came as low down as the pendulum ball, the farthest then possible from it. The weight is about 27lb.; and the pendulum, which is a mercurial one, has about 10lb. of mercury in a glass jar or hollow cylinder. In order to be more convinced in this matter, the clock was again wound up, and during the time of the first ten days or a fortnight it kept the arc of vibration constantly the same, and its rate of time was somewhat less than $+0{\cdot}1''$ per diem; when the weight had got down, and partly opposite to the cylinder of mercury, the arc of vibration began to come in, and the clock gained in the mean time from less to more, even to five seconds in one of those days; when the weight had got below the pendulum or cylinder of mercury, the former arc of vibration was regained, and its rate of time also. The full extent of the arc of vibration is about one degree eleven minutes on each side the point of rest, of this it lost about six minutes when at its least extent.

This had been foreseen, and guarded against, but not effectually.

There was another clock observed, which goes a month, has somewhat of the common scapement to it, and a compensation pendulum of Ward's form, which I think is a very excellent one. The arc of vibration is about three degrees six minutes on each side the point of rest, the weight of the pendulum ball about

Another clock affected in a similar manner.

about 12lb., and that of the going weight 28lb. From the forcible swing or motion of this pendulum, it was thought, that attraction would scarcely have any influence here; however, the weight, when opposite to the pendulum ball, brought the arc of vibration gradually in to three degrees; but on the weight leaving or getting below the pendulum ball, the former arc of three degrees six minutes was regained.

Month clocks frequently troublesome, even stopping, from this cause.

There are few clock-makers, who may not in the course of their experience have had a great deal of trouble with old month clocks, much vexation and running after them, on account of their stopping; and this apparently from causes inexplicable and undiscoverable; and with the greatest difficulty, after taking them into their hands, could they *sometimes* be made to go. Clock-makers, who may know the principles of their business tolerably well, and who would find no difficulty in making a common eight-day clock to go and perform its office easily, have often been much put to it by month clocks. They in general have very heavy weights, perhaps thirty pounds to the going part, and more than that to the striking part; the pendulum balls are very light, little more than one pound weight, if even this, and withal have very short arcs of vibration. These circumstances lead me to suspect, that attraction had had a great hand in the stopping of these clocks; and had *this* been known then, we should have found the stopping generally to have taken place about the time when the weights had got down, and nearly opposite to the pendulum ball; which is fully confirmed by my examining some old and well-experienced clock-makers on this part of the subject, who said, that this actually was the case, that they always suspected among other causes of stopping, that the weights might have touched the pendulum, and this they very frequently examined, to see if there was sufficient freedom for the weights to pass the pendulum ball without touching it, never dreaming of such a thing as attraction being there; but this of the weights being opposite to the pendulum would not be much noticed, from no suspicion being attached to them, but that of merely touching the pendulum ball. If the influence of attraction (which now must be admitted) takes place in those clocks which have heavy weights, it must take place, though in a less degree, in those clocks which go eight days; and, were it not for the trouble of daily

Thirty hour clocks have advantages over others.

daily winding up, a thirty-hour clock would, on this account of attraction, as well as for many other good reasons, be the most preferable of any.

I am, Sir,
Your most obedient Servant,
T. REID.

III.

An Essay on the apparent Figure of Stars and luminous Objects, seen at a very great Distance, and under a very small Diameter. *By Mr.* J. H. HASSENFRATZ*.

Phænomena of vision through small apertures.

IN the year 1806, I laid before the physical and mathematical class of the Institute several observations on the phænomena of vision through small apertures; and from them I have extracted the following, on the apparent figure of stars, which are here brought together in one paper.

Luminous objects seen at a distance.

If we look with the naked eye at a very remote luminous object; a star, a planet, a torch, a candle, or even a house on fire; we perceive, that these bodies are surrounded with rays of light, having particular directions; and that these rays prevent our distinguishing and ascertaining the figure of the object.

The rays from them differ to different eyes.

The number of these rays differs to different eyes: but two rays, AB, AC, pl. II, fig. 1, in the direction of the eyes, are pretty generally observed; as also a third, AD, perpendicular to them. Some distinguish a fourth ray, AE, which is a prolongation of AD: others see a fifth, AF, fig. 2: and in certain circumstances the spectator observes six or eight, fig. 3.

The commencement and progress of this phænomenon described.

When the luminous body is so near the spectator, that he can approach it, he may see this phænomenon commence and increase.

On receding from a candle to the distance of distinct vision, its figure is commonly that of a spear head, fig. 4. On receding farther, its dimensions are changed, and the flame appears broader, as at fig. 5. Lastly, on retiring still farther, it assumes the figure of a lozenge, as at fig. 6; and at a greater distance it begins to exhibit cross rays.

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* Ann. de Chim. vol. lxxii, p. 5.

The distance at which these cross rays begin to appear, is different with different people. I have always observed them, when I was twenty-five or thirty yards from a candle, and when consequently its flame was seen under an angle of one or two minutes. Some distinguish these rays at a shorter distance; others do not begin to perceive them till they are much farther off*.

The length of the rays issuing from the stars is so much the greater, in proportion to their brightness, and to the darkness of the night. The luminous rays of candles, torches, and bodies on fire, diminish in length in proportion to the intensity of their light; but we see those of the stars increase from the time of twilight, when they begin to be perceptible, to the time when the night is very dark.

Luminous objects seen through a transparent substance.

If we look through a body of clear water, or any transparent substance, at a star or bright light, that would appear to give out long luminous rays to the naked eye; we shall perceive, that the length of their rays diminishes, in proportion as the thickness of the body of water, or transparent substance, through which they are seen, is increased.

Star seen through a telescope.

A star seen through a telescope, the object glass of which, being of large diameter, increases considerably the intensity of the light at its focus, appears to be accompanied with four or more luminous rays. But if the intensity of the light be diminished, either by lessening the diameter of the object glass, intercepting a part of its light, enlarging the surface of the image, or any other method, we find the length of the rays gradually diminish, till, the intensity of the light not being sufficiently great, the rays cease to be perceptible; and then the star has the appearance of a circular disk, surrounded by a coloured aureola.

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* Is not this appearance of luminous objects seen at a distance, the origin of the figure under which stars are generally represented, though we have every reason to believe, that their form is spheroidal? Is it not their different appearance to different eyes, that has given rise to their being represented with a different number of rays? And is it not because most people perceive five rays, that they are usually delineated with this number?

Coloured aureola round a star.

The coloured aureola, that accompanies the circular image of the stars, when their light is too weak for the rays to be perceived, is independent of the cause that produces this latter phænomenon. It appears to be occasioned by the object glass being more or less imperfectly achromatic; by the faintness of the image, which permits the aureola to be distinguished; by the inflection of the ray of light at the edges of the diaphragm, that lessens the object glass; and by several other causes, on which I shall not attempt to enlarge in this paper.

Disc of a planet magnified.

As the rays that accompany luminous bodies are not perceived, unless the object producing them be seen under a very small angle, it follows, that when the diameter of a planet is increased by the assistance of a telescope, so as to be seen under an angle of some minutes, the rays disappear, and the planet is clear and well defined.

Direction of the rays depends on the eye.

In these rays there is this particularity, that their direction always depends on that of the eyes looking at the object. Thus, if, when looking at a distant light, we incline the head, as at fig. 7, we shall immediately perceive the direction of the rays change. One of these directions is constantly parallel with the two eyes, moving with them; and the others preserve their relative situation with respect to this.

The rays produced by the visual organ.

The rays distinguished round luminous objects may be produced, either by the luminous body itself, or by the organ that discerns them. In the first case, the number and position of the rays should be the same to every spectator: but, as the number of these rays varies to different eyes, and follows the direction of the eyes when these are inclined in looking at the object, it follows, that the rays are produced by the organ perceiving them.

They are destroyed by looking through a small hole.

This truth is farther confirmed by looking at stellate lights through a small aperture; as this immediately destroys the rays, and the luminous bodies appear of smaller dimensions. In this case their figure is altered only by the inflection of the light at the edges of the small aperture; an inflection which generates aureola round the images of luminous objects.

How does the eye produce them;

Since the rays, that appear to emanate from luminous objects seen at a great distance, are produced by the eye that perceives them;

this appearance?

them; it remains to ascertain, for the explanation of the phænomenon, how this appearance may be produced, and what part or parts of the organ produce it.

Rays caused by the lachrymal fluid are of a different kind.

The lachrymal fluid, by which the cornea is constantly covered, gives rise to the perception of several rays, when, in winking the eyes, the two lids approach the iris: but these rays are essentially different from those here considered, to which the name of *irradiation* has been given. The rays produced by the lachrymal fluid are all perpendicular to the direction of the eye-lids, and are produced only when these are brought very near together. They are seen sometimes at top only, sometimes at bottom, sometimes both at once, ; and this according to the position of the eye-lids with respect to the iris: and they are perceived at all distances from the luminous object. Irradiation, on the contrary, is discerned only when the light is at a very great distance, and seen under a very small angle. The separation of the eye-lids has no influence on this phænomenon: it is always perceived, however wide they are asunder: and lastly the rays are seen in four, five, six, or eight directions, one of which is always parallel to the eye-lids.

The cause not in the retina, or aqueous or vitreous humour.

As the production of this phænomenon cannot be ascribed to the retina that covers the bottom of the eye, and receives the image; or to the aqueous and vitreous humours, through which the pencil of light passes; we have every reason to believe, that it is owing to the action of the cornea, or of the crystalline, or to the action of both conjointly.

Effect of the cornea and crystalline.

The cornea and crystalline, by the nature of the curvature of their surface, refract the divergent rays that arrive at the eye, and cause them to converge to a particular focus. Now it is demonstrable by analysis, and may be verified by experiment, as I have ascertained, that if the surfaces separating mediums be segments of a sphere, the image produced by the rays emanating from a luminous object, and received on a plane perpendicular to the axis of the pencil is always a circle: but if the curved surface, convex toward the least refracting medium, be generated by two different osculatory radii, the image is formed of two ellipses, which intersect each other at an angle depending on the position of these two radii. If, therefore, the surfaces of the cornea, or of the crystalline, be not segments of a sphere, this is sufficient to cause the image, formed at the bottom

Effect of a compound curvature of a refracting surface.

tom of the eye by the light traversing this organ, to approach so much the more the form of a cross, in proportion as the luminous object is more remote from the eye, and as the two radii, generating those surfaces, differ more from each other.

Ellipsoidal lens.

The two ellipsoidal images decussating each other, fig. 8, are constantly seen, if a ray of solar light, the light of a candle at a distance, &c. be made to pass through an ellipsoidal lens. The same image, too, is observable, if they pass through irregular surfaces; such, for instance, as phials or decanters filled with water, &c.

Figure of the surface of the cornea difficult to ascertain.

It is extremely difficult to ascertain with precision the figure of the surface of the cornea in the living subject. After death the cornea becomes flaccid, and undergoes alterations that prevent us from distinguishing accurately the nature of the surface it had. Attempts were made to ascertain the figure of the eye by freezing it: but the increase of bulk of the fluid by congelation so altered it, that it was impossible to form a precise idea of the nature of its curved surface.

Apparent irregularity in it.

Petit's opinion.

Probably not spherical.

On looking at a fresh human cornea, it appears to be of an irregular figure, but this irregularity is occasioned, in great measure, by the projection of the tunica conjunctiva over the upper part of the cornea. Dr. Petit, who paid great attention to the figure and dimensions of the eye, says, in a paper published among those of the Academy of Sciences, in 1726, that, when the portion of the conjunctive coat advancing upon the cornea is dissected off, the latter is commonly round: yet he met with the cornea of a negro, that measured 5·5 lines French from right to left, and only four from top to bottom. I have found it hitherto impossible, to obtain accurate data; all my observations, however, lead me to believe, that it is not spherical.

The crystalline.

As the crystalline may easily be separated from the eye, physiologists could not fail to make observations on it. Accordingly, all who have treated on the organ of sight have been ready to describe the figure and composition of the crystalline.

Different accounts of its form.

Galen considers the crystalline as not being a perfect sphere, uniform throughout its whole extent; but approaching to a compressed globe, fig. 9.

Rufus of Ephesus thinks, that from its figure it should be called lenticular.

Theophilus asserts, that the interior surface of the crystalline is less convex than the exterior: fig. 10. Fallopius, Zinn, and many other anatomists, are of the same opinion.

Vassalli says, he has observed the convexity equal on both surfaces: fig. 11.

Brisseau asserts, that the surface of the crystalline next the cornea is more convex, than that which is in contact with the vitreous humour.

Petit, who had seen and observed a great number of crystallines, says he observed some to have the anterior curvature greater than the posterior, fig. 12; but that most commonly the side toward the cornea had a radius of curvature greater than that in contact with the vitreous humour, fig. 10. He says even, that he found in several subjects the crystallines to have their greatest curvature, one at the anterior surface, the other at the posterior; and that he has met with crystallines both surfaces of which were equal, fig. 11.

Those anatomists agree in considering the two surfaces of the crystalline as two segments of a sphere applied to each other, fig. 13.

Dr. Thomas Young concludes from observations made on his own eyes, that the anterior surface of the crystalline must be a portion of an hyperboloid, and the posterior surface a portion of a paraboloid: but he admits, that his experiments would not succeed equally with every eye.

Dr. Petit says, he has met with crystallines, the posterior convexity of which was not spherical, but approached the paraboloid form.

Its figure varies with age. All persons, who have made observations on crystallines, know that their figure varies with age. Those of infants are small and thick. In some fœtuses the thickness is but little less than the breadth. Those of adults are about twice as broad as they are thick. Those of old men grow flatter and yellow.

Its dimensions. Petit has observed, that the dimensions of the crystalline are not always proportional to the age; and though in general the breadth is 9 mil. [3·54 lines], and the thickness 4·5 [1·77 lines], he has found it from 5 to 8 thick [1·97 to 3·15*l.*], and even 9 [3·54*l.*].

Thus, there is not only a want of agreement between anatomists respecting the figure of the crystalline; but they who have observed it with the greatest care, as Petit, find very great differences in it; differences that must necessarily affect vision, and produce in great measure those variations, which have been noticed in the sight of different persons by physiologists and natural philosphers.

It differs greatly.

Whatever care has been taken to ascertain the figure of the crystalline, observers hitherto appear to have attempted only to determine the proportions that exist between the versed sines of the curvature of the segments, AB, AC, fig. 14, and the length of their chord, DE. No one that I know has endeavoured to ascertain, whether the plane of the posterior and anterior segments were circular; and whether there existed any difference between its diameter from right to left DE, and its height GF. This difference appeared to them not sufficiently perceptible to be measured.

Its anteriorand posterior surfaces supposed to unite in a circular line.

However, as there are some crystallines, the horizontal and vertical diameters of which exhibit a pretty considerable difference, these could not escape an accurate observer. Thus Petit, in a paper read to the Royal Academy of Sciences in 1730, says: "the circumference of the crystalline is commonly round; yet I have found some in the human subject, that were not so, and the diameter of which was a quarter of a line longer one way than the other."

This not always the case according to Petit.

In this paper Petit describes a great number of observations made on the crystallines of various animals; those of man being introduced only as forming one of the links of the great chain.

He examined it in various animals.

As Petit is the only person, who has measured crystallines with sufficient care to perceive, that those of man are not round; and to observe, that one of the diameters exceeded the other by a quarter of a line; I thought it might not be amiss to repeat these experiments, in order to satisfy myself whether this particular observation of Petit was sufficiently general, to contribute to the production of the irradiation; and at the same time ascertain the directions in which the longer and shorter diameter are placed.

The examination repeated.

I immediately procured two sheep's eyes, which I opened cautiously. On taking out the crystallines, and laying them flat, I perceived, that the curve uniting the two segments was longer in one direction than in the other.

Sheep's eyes.

2. That these figures are altered, in proportion as we recede from this; and that at a great distance, when these objects are seen under an angle of one or two minutes, they appear surrounded with several irradiations, two of which are in the direction of the eye-lids.

3. That these irradiations are independent of the figure of the luminous object, and are produced by the organ perceiving them.

4. That these irradiations are occasioned chiefly by the irregular figure of the surfaces of the crystalline and cornea.

5. Lastly, that this irradiation is not well distinguished, except in the dark; because, the iris having then a greater opening, the irradiation occasioned by the irregularity of the surfaces of the crystalline and cornea becomes more perceptible.

IV.

On the double Refraction of Light in transparent Crystals: by Mr. Laplace.*

Refraction of light.

LIGHT, when it passes from the air into a transparent medium not crystallized, is refracted so that the lines of incidence and refraction are constantly in the same ratio: but in passing through most diaphanous crystals it exhibits a singular phænomenon, which was first observed in Iceland crystal, where it is very perceptible.

Double refraction.

A ray of light, falling perpendicularly on one of the natural faces of this crystal, divides into two parts; one traversing the crystal without altering its direction; the other deviating from it in a plane perpendicular to that face, and passing through the axis of the crystal, that is, through the line that unites the summits of its two obtuse solid angles. This division of the ray generally takes place with regard to any face, natural or artificial, and whatever be the angle of incidence; one portion following the law of common refraction, the other a law of extraordinary refraction first discovered by Huygens; and which, considered as the result of experiment, may be classed among the

Law of the extraordinary refraction discovered by Huygens.

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* Journal des Mines, vol. XXIV, p. 401.

the finest discoveries of that eminent genius. He was led to it by the idea he had formed of the propagation of light, which he supposed to be produced by the undulations of an ethereal fluid. According to him, the velocity of these undulations was less in common transparent mediums, than in a vacuum, and the same in all directions. But he supposed there were two kinds of undulations in Iceland crystal: and that the velocity of one was the same in all directions, as in ordinary mediums; but that the velocity of the other was variable, and represented by the radii of an oblate ellipsoid of revolution, the centre of which was at the point of incidence of the luminous ray on the face of the crystal, and the axis parallel to the axis of the crystal. Huygens had also found, that, to render the hypothesis answerable to experiment, the velocity of the undulations respecting the *ordinary* refraction must be represented by half the conjugate axis of the ellipsoid; which connects in a very remarkable manner the two refractions, the ordinary and extraordinary. This great geometrician did not assign the cause of this variety of the undulations; and the singular phænomenon exhibited by the light in passing from one crystal to another, which will be noticed at the end of this paper, is inexplicable on his hypothesis. This, added to the great difficulties offered by the theory of waves of light, occasioned Newton, and most of the philosophers who have followed him, to reject the law of refraction, that Huygens had attached to it. But Mr. Malus having proved the precision of this law by a number of very accurate experiments, we should separate it altogether from the hypothesis that led to its discovery*. It would be very interesting to connect it, as Newton has done ordinary refraction, with the attractive or repulsive forces, the action of which is sensible only at imperceptible distances. It is, in fact, very probable, that it depends on them; and I have satisfied myself of it by the following considerations.

His hypothesis of the propagation of light.

His law rejected:

but Malus has proved its accuracy.

The result of attraction.

The principle of a minimum of action generally takes place in the motion of a point subjected to forces of this kind. On applying this principle to light, we may set aside the imperceptible

Principle of least action applied to light.

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* Dr. Wollaston had before shown the hypothesis of Huygens to be agreeable to experience: See Phil. Trans. for 1802, or Journal, vol. IV, p. 148. C.

ceptible curve it describes in its passage from a vacuum into a transparent medium, and consider its velocity as constant, when it has entered into it by a perceptible quantity. The principle of a minimum of action then is reduced to this, that the light arrives from a point without the crystal to a point within it, in such a manner, that, if we add the product of the right line it describes without multiplied by its primitive velocity, to the product of the right line it describes within multiplied by its corresponding velocity, the sum will be a *minimum*. This principle always gives the velocity of light in a transparent medium, when the law of refraction is known: and reciprocally it gives this law, when the velocity is known. But a condition to be fulfilled in the case of extraordinary refraction is, that the velocity of the luminous ray in the crystal shall be independent of the manner in which it entered, and depend only on its position with respect to the axis of the crystal, that is, on the angle which it forms with a line parallel to the axis. In fact, if we imagine an artificial face perpendicular to the axis, all the interior extraordinary rays, that are equally inclined to this axis, will be so likewise to the face, and will evidently be subjected to the same laws at issuing from the crystal: all will resume their primitive velocity in the vacuum; the velocity in the interior therefore is the same for all. I have found, that the law of extraordinary refraction given by Huygens fulfils this condition, as well as it does that of the principle of a minimum of action; which leaves no room to doubt, that it is owing to attractive and repulsive forces, the action of which is sensible only at imperceptible distances. Hitherto it could only be considered as approaching it within limits less than the inevitable errours of experiment: now it may be taken as a precise law.

Case of extraordinary refraction.

Law of Huygens.

A valuable datum for the discovery of the nature of the forces that produce it is the expression of the velocity, to which analysis has conducted me; and which I find equal to a fraction, the numerator of which is unity, and the denominator of which is the radius of the preceding ellipsoid, according to which the light is directed, the velocity in vacuo being taken as unity. I show, that the velocity of the ordinary ray is unity divided by the semiaxis of revolution of the ellipsoid; and by these

Expression of the velocity.

these means the very remarkable connexion, that Huygens found by experiment, between the ordinary and extraordinary refractions of the crystal, is demonstrated *a priori*, as a necessary result of the law of extraordinary refraction. The velocity of the ordinary ray in the crystal therefore is always greater than that of the extraordinary ray, the difference of the squares of the two velocities being proportional to the square of the sine of the angle that the axis forms with the latter ray. According to Huygens, the velocity of the extraordinary ray in the crystal is expressed by the radius of the ellipsoid itself; his hypothesis therefore is conformable to the principle of least action: but it is remarkable, that it is also conformable to the principle of Fermat, which consists in this, that the light arrives from a given point without the crystal to a point within in the shortest time possible; for it is easy to see, that this principle is reduced to that of the least action, by reversing the expression of the velocity. Thus the law of refraction given by Huygens is deducible equally from both of these principles. For the rest, this identity of the laws of refraction, deduced from the mode in which Huygens viewed the refraction of light, with those given by the principle of least action, takes place generally, whatever be the spheroid, the radii of which, according to him, express the velocity of the light in the interior of the crystal. This I demonstrate very simply in the following manner.

Difference of the velocities of the two rays.

Principle of Fermat.

Construction of Huygens.

Huygens considers a ray R C, pl. II, fig. 16, falling on the natural or artificial face A F E K of an Iceland crystal. Drawing a plane, C O, perpendicular to this ray, and taking O K, parallel to C R, to represent the velocity of light in vacuo, he supposes, that all the points C o o' O of the luminous wave arrive in the same time, and in parallel directions, at the plane K i' i I; which he finds thus. A F E D is an ellipsoid of revolution, of which C is the centre, C D the semiaxis of revolution; and the radii of which represent, according to Huygens, the respective velocities of the light that follows their directions. Through the ray R C he draws a plane perpendicular to the face, and cutting it in the right line B C K; and through the point K he draws, in the plane of the face, K T, perpendicular to K C. Lastly, through K T he draws a plane K I, touching the ellipsoid in I. According to him C I is the direction

tion of the refracted ray. In fact it is easy to show, that in this construction any given point o of the luminous wave arrives at i, through the broken line $o\,c\,i$, in the same time as O arrives at K. C I representing the velocity of the refracted ray, the right line C I is traversed in the same time as the right line O K. Let us take this for the unit of time, and O K for the unit of space. The point o arrives at c in a time proportionate to $o\,c$, and consequently equal to $\frac{Cc}{KC}$. It passes from c to i in the interior of the crystal, in a time equal to that which the light employs in passing from C to I multiplied by $\frac{Kc}{CK}$, and consequently equal to $\frac{Kc}{KC}$, $c\,i$ being parallel to C I. By adding this time to $\frac{Cc}{KC}$ we shall have unity for the time that the point o employs in arriving at i.

Let us take $o'c'$ infinitely near to $o\,c$, and parallel to it, the point o' will arrive at i in the unit of time. Draw the right lines $c'o$ and $c'i$, and suppose, that the point o proceeds to i through the broken line $o\,c'\,i$. Now $c'o'$ being perpendicular to C O, the right line $c'o$ may be supposed equal to $c'o'$, and the times required to pass through them may be supposed equal. Moreover, the time required to pass through $c'i$ may be supposed equal to the time required to pass through $c'i'$, because, the plane K I touching in i the spheroid similar to the spheroid A F E D, the centre of which is in c', and the dimensions of which are diminished in the ratio of K c' to K C, the two points i and i' may be supposed in the surface of the spheroid. According to Huygens the velocities according to $c'i$ and $c'i'$ are proportional to these lines; the times employed in passing through them therefore are equal. Thus the time of the transmission of the light in the broken line $o\,c'\,i$ is equal to unity, as in the broken line $o\,c\,i$: the differential of these two times therefore is null, which is the principle of Fermat.

The reasoning applicable to any spheroid.

It is clear, that this reasoning is generally applicable, whatever be the nature of the spheroid, and the position of the points c and c' on the face of the crystal; even if they be not in the right line CK, provided they be infinitely near it.

The hypotheses of Huygens, though false, represent the fact.

Reversing the expression of the velocity, the principle of Fermat gives that of the least action. The laws of refraction arising from the hypotheses of Huygens, therefore, are generally conformable to this latter principle; and for this reason these hypotheses, though erroneous, represent the fact.

If we put b= the semiaxis of revolution of the ellipsoid of Huygens, a= its semitransverse axis, o= the velocity of a ray of light in the interior of the crystal, and V= the angle its direction makes with the axis, the radius of the elipsoid will be $\frac{a\,b}{\sqrt{a^4-(a^2-b^2).\ \sin^2.\ V}}$. Thus the velocity v, from the principle of least action, being equal to unity divided by this radius, we shall have $v^2=\frac{1}{b^2}-\left(\frac{1}{b^2}-\frac{1}{a^2}\right).\ \sin^2.\ V.$

This velocity is least when the ray of light is perpendicular to the axis of the crystal, and then it becomes $\frac{1}{a}$: it is least, when it is parallel to this axis, and then it is equal to $\frac{1}{b}$.

Connexion between the two refractions.

Huygens found by experiment, that b is the ratio of the sine of refraction to the sine of incidence in the common refraction of the Iceland crystal. This very remarkable result, which connects the ordinary and extraordinary refractions, is a necessary consequence of the modifications that distinguish the ordinary from the extraordinary ray not being absolute, but solely relative to the position of the ray with respect to the axis of the crystal. To show this, let us refer to the singular phenomenon, that light exhibits after its passage through a crystal.

Phenomena of light after its passage through a crystal.

In passing through a crystal the light is divided into two pencils, one ordinary, the other extraordinary, and each of them issues out of the crystal undivided. If we conceive a second crystal placed beneath the first, in a situation perfectly similar, the ordinary ray will be refracted ordinarily on passing into the second crystal, and the extraordinary ray will be refracted extraordinarily. This will take place generally, if the principal sections of the two opposite faces be parallel. By the *principal section* of a face is meant a section of the crystal by a plane perpendicular to that face, and passing through the axis of the crystal. But, if the principal sections be perpendicular to each other, the ordinary ray will be refracted extraordinarily on passing into the second crystal, and the extraordinary ray will be refracted ordinarily. In the intermediate positions, each ray will be divided into two others at its entrance into the second crystal.

Now suppose a ray refracted ordinarily by one crystal to fall perpendicularly on a second crystal cut by a plane perpendicular to its axis: it is clear, that an infinitely small inclination of the axis to the face of incidence will be sufficient to change this ray into an extraordinary ray. But this inclination can produce but an infinitely small change in the action of the crystal, and consequently in the velocity of the ray within it: this velocity, then, is that of the extraordinary ray, and consequently it is equal to $\frac{1}{b}$: which comes to the same as the result of Huygens: for it is known, that the velocity of light, in common transparent mediums, expresses the ratio of the sines of incidence and refraction, its velocity in vacuo being taken as unity.

Reflection of light.

The principle of least action may serve also to determine the laws of the reflection of light; for, though the nature of the force, that causes light to rebound from the surfaces of bodies, is unknown, it may be considered as a repulsive force, which restores, in a direction contrary to that of the light, the velocity it causes it to lose; as elasticity restores to bodies in a contrary direction, the velocity which it destroys. Now we know, that, in this case, the principle of least action always subsists. With respect to a luminous ray, whether ordinary or extraordinary, reflected by the exterior surface of a body, the principle is reduced to this, that the light passes from one point to another by the shortest path of all those that fall in with the surface. In fact, the velocity of reflected light is the same as that of direct light: and it may be laid down as a general principle, that, when a ray of light, after having experienced the action of as many forces as you please, returns into a vacuum, it resumes its original velocity. The condition of the shortest path gives the equality of the angles of reflection and incidence in a plane perpendicular to the surface, as Ptolemy had already remarked. It is the general law of reflection at the external surfaces of bodies.

Reflection in the case of double refraction.

But when light, on entering into a crystal, is divided into ordinary and extraordinary rays, one portion of these rays is reflected by the interior surface at their exit from the crystal. In being reflected, each ray, whether ordinary, or extraordinary, divides into two others; so that a solar ray, penetrating the crystal, forms by its partial reflection at the surface of emission four distinct pencils, the direction of which I shall proceed to determine.

Let

Case of parallel surfaces.

Let us suppose the surfaces of entrance and emission, which we will call the first and second faces, to be parallel to each other. Let the thickness of the crystal be imperceptible, yet greater than the sum of the radii of the spheres of activity of the two faces. In this case it will be demonstrated, by the preceding reasoning, that the four reflected pencils will form but one perceptibly, being in the plane of incidence of the generating ray, and forming with the first face an angle of reflection equal to the angle of incidence. Now let us restore the crystal to its proper thickness: it is clear, that, in this case, the reflected pencils, after issuing from the first face, will assume directions parallel to those they had taken in the former case: these pencils, therefore, will be parallel to each other, and to the plane of incidence of the generating ray; only, instead of being confounded to the senses, as in the former case, they will be separated by distances so much the greater, as the crystal is thicker.

Now, if we consider any given interior ray issuing out in part by the second face, and in part reflected by it into two pencils, the issuing ray will be parallel to the generating ray; for the light, as it issues out of the crystal, must take a direction parallel to that it had on entering into it; since, the faces of entrance and exit being supposed parallel, it is acted on at its exit by the same forces as it was at its entrance, but in the opposite direction. In the direction of the issuing ray, let us conceive a plane perpendicular to the second face; and in this plane let us imagine a right line, exterior to the crystal, passing through the point of exit, and forming with the perpendicular to the face, but on the side opposite to the direction of the issuing ray, the same angle as that direction; lastly, let us conceive a ray of light entering the crystal according to this right line. This ray, at its entrance, will be divided into two others, which, at issuing out of the crystal by the first face, will take directions parallel to that of the ray before its entrance by the second face; they will be visibly parallel to the directions of the two reflected pencils; which cannot take place but as far as the two rays, into which the ray of light is divided on entering by the second face, confound themselves respectively in the interior of the crystal with the directions of the two reflected pencils. But the law of Huygens gives the directions of the rays into which

The law of

Huygens applicable here, which the ray of light is divided; therefore it will give those likewise of the two pencils reflected in the interior of the crystal.

and in the case of surfaces not parallel. If the two faces of the crystal be not parallel, we shall have by the same law the directions of the two rays, into which the generating ray is divided in entering by the first face: we shall have also, by this law, the direction of each of these rays at its exit by the second face: next, the preceding construction will give the directions, in the interior of the crystal, of the four pencils reflected by this face: and, lastly, by the law of Huygens we may deduce their directions at issuing out of the crystal by the first face. Thus we shall have all the phenomena of the reflection of light by the surfaces of transparent crystals. Mr. Malus first discovered these laws of the reflection of light, and he has confirmed them by a great number of experiments. Their agreement with the results of the principle of least action completes the demonstration of the position, that all these phænomena are owing to the action of attractive and repulsive forces*.

V.

Description of a Reflecting Circle, in which the Screens can be readily shifted in taking altitudes: by Mr. J. ALLAN, *Blewitt's Buildings, Fetter Lane.*†

SIR,

Improved reflecting circle. I BEG leave to inform you, that on Thursday last I left at the Society's house a mathematical instrument, adapted for the use of mariners, which I wish to submit to the Society's attention. It is a reflecting circle, commonly called, La Borda's circle, for the purpose of taking altitudes and distances at sea; and which I have greatly improved lately, by fixing the shade glasses different to what had heretofore been done, with some other improvements as a reflecting circle. The late Dr. Mackay, in a in a publication of his, called Mackay's Longitude, has a plate of La Borda's original instrument, but the shade glasses are so fixed, as to render the instrument useless, and which he was convinced of, on my pointing out to him the fault. He said he would alter his plate to my method, and that he would state it as my improvement; but his death soon afterwards prevented it. I am aware the Society do not confer their rewards without advantageous qualities to merit their sanction. I respectfully say, that I consider my instrument to have merit, both in economy, and in the great improvement made on the plan of the reflecting circle first invented. I shall be happy to point out this to the Society, and have the honour to be,

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* For the two papers of Mr. Malus on this subject, see Journal, vol. XXX, pp. 95 and 161.

† Trans. of the Soc. of Arts, &c. vol. XXIX, p. 106. The silver medal and twenty guineas were voted to Mr. Allan for this improvement.

Defect in La Borda's.

Sir, your humble servant,

JAMES ALLAN.

Blewitt's Buildings, Fetter Lane,
Dec. 24th, 1810.

SIR,

Agreeably to the intimations of the committee on Thursday evening last, I beg leave to explain to the Society the properties of my improved reflecting circle; and which, with a theodolite attached to it, would be useful both to the mariner and surveyor.

Useful both to mariners and surveyors.

The committee inquired what sort of centre or axis the instrument had. I beg leave to state, it is an improved one of mine. The former way of centring this instrument was only by a single pin, which both indexes acted upon; but the pin had so little bearing in the index, that it was not sufficient to keep the index-glass upright to the plane of the instrument in all its positions; I have therefore contrived to put what is called in our business a male and female centre or axis, upon a simple but accurate method.

Mode of centring.

Permit me to make a few observations on circular instruments in general. I believe it will be universally allowed, that it is easier to make a circle nearer to truth, with respect to its horizontal plane, than it is to make a separate part of a circle so.

Horizontal plane of a whole circle,

A sextant is only the sixth part of a circle, and is got flat by means of a plane, as near as the maker can get it, but is not turned on its own axis as a circle is: therefore I have no doubt, but that the best sextant usually made is very short of the

made truer than of a part.

the horizontal truth of a sixth part of a circle; and if we were to suppose a circle made of six of the usual sextants, it would be a very untrue circle with respect to its horizontal plane.

Reflecting circles made in various ways.

It has, therefore, been a general desideratum, that a circular instrument of reflection should be introduced, of simple construction, easy to adjust, and convenient for use. I have been induced to make several circular instruments of reflection in various ways, but none upon so simple a construction, or so cheap as the present, nor so well calculated to prove any untruth, as my improvement upon Borda's; and I believe it will now be generally adopted for use.

Borda's unsatisfactory.

There have been great numbers of Borda's circles made; I myself assisted about twenty-five years ago to make many, also since I have been in business for the last twelve years on my own account, but I never found any of them to give satisfaction till I invented the present improvement.

Captain M'Lennan, who traded to South America, had one of Borda's circles made, similar to that described in Dr. Mackay's Longitude, but could not use it till altered by me last April.

The improved circle well adapted for proving its correctness.

The glasses in my instrument are movable to any quarter that a person may wish to use it in; and by taking the same angle with each quarter, it affords an opportunity of proving the correctness of the instrument, which circumstance I hope justifies me in saying, that it is the only instrument of reflection that I know, so well calculated to prove itself. I beg pardon for being so tedious; I assure you that I can make the instrument better than I can write or talk about it.

I have the honour to be, Sir,
Your humble servant,
JAMES ALLAN.

Blewitt's Buildings, Jan. 16, 1811.

Testimonials in its favour.

A certificate was produced from Captain H. C. Coxen, R.N. dated February 5th, 1811, stating his opinion, that the dark screens which are fixed to Mr. Allan's reflecting circle, so as to act in the manner they do in a sextant, are improvements on the reflecting circle of Mr. Borda, which are not so fixed.

That it must be evident, even to the least experienced mariner, that there are frequent occasions, in taking the altitude of the sun, to change the screens *alternately*, in the shortest possible time, which cannot be effected in near so short a time by screens which take off and on, as in Borda's reflecting circle.

Captain Mackay, who has commanded the Lord Forbes, in the Jamaica trade, for twenty years, stated, that the manner in which Mr. Allan's screens are fixed in his reflecting circle is a great improvement. That from not being obliged to take out the shades when the sun is clouded, the object is not lost; and that when an instrument is obliged to be taken from the eye, to fix the screen in the old mode, the object is lost.

That by this instrument being a reflecting circle, it makes sure of a horizontal plane well divided, which can hardly be the case in a portion only of a circle.

That Mr. Allan's is the most complete instrument he has ever seen, and that he shall always take one with him to sea.

Description of the Drawing of Mr. James Allan's Improvement on the Reflecting Circle of Borda. Pl. III.

Description of Mr. Allan's improved reflecting circle.

The reflecting circle, first invented by Tobias Mayer, of Gottingen, and afterwards improved by the chevalier La Borda, of Paris, is an instrument, which in its principle admits of such a degree of accuracy, as to be of the most important service to navigators; but it has hitherto been constructed in such a manner, that the inconveniences attending the use of it have prevented its general adoption among seamen: any contrivances, therefore, tending to diminish these inconveniences, were deserving of the Society's notice. The construction of Borda's circle, as it has hitherto been made, is minutely detailed in Dr. Rees's New Cyclopædia, article, Circle; and the mode of using it is there explained; it will be therefore unnecessary to describe any thing more of the circle delineated in pl. III, than is essential to the elucidation of the improvements made by Mr. Allan.

Application of the dark glasses.

The first of these is in the mode of applying the dark glasses, which are fixed on joints, so as to turn back out of the way, in the same manner as in the sextant. In the old instrument these glasses were fitted into sockets provided with tenons on the indexes, and fastened by a milled head screw, which took much time to change them. The second is the addition of

Double verniers. double verniers to the index, carrying the telescope and horizon glass; these read upon opposite sides of the circle, and if a difference is observed between these readings, by taking the mean of them the errour arising from any eccentricity the index may have, will be corrected. Centring of the index glass. And the third consists in fixing the index-glass upon an axis, accurately fitted into the centre of the circle. By this means it is assured, that the index glass, in turning round, shall always be exactly perpendicular to the plane of the circle. In the old method, when the index-bar was merely fitted on a pin fixed in the centre of the circle, it was impossible to make the circle so perfectly flat, or keep the index so accurately in contact with it, as by having an axis. Explanation of the plate. To explain these improvements more perfectly, the reader is referred to plate III, which contains a perspective view of the instrument; A, is the circle with six arms; B, is the index carrying the telescope C, and the horizon-glass D, with the two clusters of dark glasses E and F. At the opposite ends of this index are the two verniers *a* and *b*; the former has the clamp screw and slow movement attached to it; consisting of a screw *c*, which fixes the index to the circle; and *d* the tangent screw, which will move the index a small quantity when turned, to adjust it accurately. G is the index mirror screwed upon the index H, which has also a vernier, and a clamp and tangent screw *e e*, similar to the other. I is the handle by which the instrument is held when in use; it is fitted to a socket K, which is screwed to the centre of the circle, and is unscrewed from the circle when packed away. The handle is fitted to a springing socket, so as to turn round upon the socket K, that it may be turned to any side of the circle for the convenience of holding it; it may be fastened by a small milled nut, seen in the figure, which binds the ends of the spring socket together. L is a magnifying glass for the purpose of reading the divisions of the verniers; it is fitted upon a pin screwed into the indexes, and may be applied to either. The figure 2 in the corner of the plate is a section, showing the construction of the central part of the circle, where M is a section of the thickness of the circle, with a hole through the centre, and a recess turned out in the lower side to receive a centre piece N, which is fixed in with three small screws; a hole is turned in the centre of this piece, and an axis O is fitted into it with the utmost accuracy; this axis has a flanch on the upper end, by which it is screwed to the index H, and upon this, the under glass G, fig. 1, is fastened, by other screws passing through a piece projecting from the back of it. The axis is held in its place by a collet *r*, fitted on a square part of it, and held fast by a screw *s*; beneath this a piece is fixed on in the centre of the circle, the edge of its flanch being shown by *t* in fig. 1; it is part of the screw which holds on the spring socket K, for the handle I. The upper end of the centre piece N, which comes up above the circle, is turned extremely true, and upon this the index B is fitted, or rather a brass ring *v* screwed to it, so as to turn round upon it as a centre.

The telescope C is fixed to the index by two cocks and by two screws XX, in these it can be raised up or lowered, to adjust the different brightness of the two objects seen in the horizon glass D, the one reflected from the central mirror G, and the other seen directly through it. The dark glasses at E are intended to moderate the light of the sun, in passing from the index to the horizon glasses; the frames containing these glasses have holes E through them, to see through the telescope and horizon glass; the other dark glasses, F, are situate behind the horizon glass D, and may be turned up or down, as occasion requires.

The instrument is used in the same manner as the common reflecting circle; the angle being first taken on one side of the parallelism of the glasses, and then on the other; so that the angle is doubled; then it is repeated on a fresh part of the circle, as many times as the observer thinks proper, and the product divided by the number of observations taken. The mode of taking these observations is explained at full in Dr. Rees's Cyclopædia, and in Dr. Mackay's publication on the means of finding the longitude.

VI.

VI.

METEOROLOGICAL JOURNAL,

		BAROMETER.			THERMOMETER.					
1812.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain.	
7th Mo.										
JULY 30	N W	29·96	29·80	29·880	61	46	53·5			☽
31	S W	29·96	29·80	29·880	64	51	57·5	·20	·08	
8th Mo.										
AUG. 1	Var.	29·86	29·80	29·830	61	53	57·0	—	·02	
2	N E	29·86	29·80	29·830	64	54	59·0	—	·10	
3	Var.	29·85	29·80	29·825	63	52	57·5	—	·02	
4	N E	29·90	29·85	29·875	65	50	57·5	—	·32	
5	S W	29·95	29·90	29·925	57	50	53·5	·29	·23	●
6	Var.	30·00	29·95	29·975	63	47	55·0	—	·41	
7	N W	30·00	29·95	29·975	61	49	55·0	—		
8	N W	29·96	29·94	29·950	57	51	54·0	·17	·03	
9	N W	29·97	29·96	29·965	57	45	51·0	—		
10	N W	29·97	29·96	29·965	58	53	55·5	—	·02	
11	Var.	30·07	29·96	30·015	63	49	56·0	—	·05	
12	N E	30·14	30·07	30·105	57	44	50·5	·18		☾
13	N	30·15	30·14	30·145	64	43	53·5	—		
14	N E	30·15	30·12	30·135	67	49	58·0	—		
15	E	30·12	30·07	30·095	65	50	57·5	—		
16	E	30·06	30·05	30·055	68	54	61·0	·34		
17	S E	30·05	29·98	30·015	73	55	64·0	—		
18	S E	29·98	29·76	29·870	78	58	63·0	—		
19	S W	29·96	29·76	29·860	72	55	63·5	·56	·01	
20	W	30·00	29·98	29·990	71	55	63·0	—		○
21	S W	29·97	29·94	29·955	69	57	63·0	—		
22	W	30·05	29·97	30·010	68	53	60·5	—		
23	S W	30·04	29·86	29·950	70	60	65.0	·66		
24	S W	30·10	29·86	29·980	66	47	56·5	—	·05	
25	S W	30·10	30·04	30·070	69	52	60·5	—		
26	N W	30·04	29·96	30·000	67	53	60·0	·35		
		30·15	29·76	29·968	78	43	57·91	2·75	1·34	

The observations in each line of the table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

NOTES.

REMARKS.

Eighth Month. 4. Wet afternoon. 5. Wet morning. 6. "The day was gloomy : about 4. p. m. a very heavy shower commenced, which continued for about 20 minutes, then abated for a short time, but increased again, and continued all the evening, with thunder and lightning : the barometer was nearly stationary." Such were the phænomena at the laboratory, where there fell 1·39 inches of rain. At Plaistow, two miles distant, there appears to have fallen only 0·41 inches of rain, and I find only this note, "Thunder in the afternoon." 13. Foggy morning : a *stratus* at night. 14. The same. 17. The same : Lunar halo. 18. Some lightning during the night. 21. Thunder between one and two p. m. 24. Bright moonlight. 28. The wind this night very high. 30. Very showery.

RESULTS.

Prevailing winds westerly.

Barometer : highest observation 30·15 inches ; lowest 29·76 inches ,
Mean of the period 29·968 inches.

Thermometer : highest observation 78° ; lowest 43.
Mean of the period 57·91°.

Evaporation 2·75 inches. Rain 1·34 inches.

PLAISTOW. L. HOWARD.
Ninth Month, 15, 1812.

P. S. The observations on the barometer, wind, and evaporation, with the remarks, for the last two months, are chiefly due to my friend, John Gibson. The account of temperature and rain was carefully kept (during my absence) at Plaistow.

VII.

An Account of some Experiments on the Combinations of different Metals and Chlorine, &c. By JOHN DAVY, *Esq. Communicated by Sir* HUMPHRY DAVY, *Kt. LL. D., Sec. R. S.*

(Concluded from p. 21.)

4. *On the Combinations of Chlorine with Manganese, Lead, Zinc, Arsenic, Antimony, and Bismuth.*

Only 1 compound with some metals.

I HAVE attempted, by several methods, to obtain more than one combination of these different metals and chlorine, but without success.

Compound of oximuriatic acid and manganese.

I have procured a compound of manganese and chlorine, by evaporating to dryness the white muriate of this metal, and heating to redness the residue in a glass tube, having only a very small orifice. Muriatic acid vapour was produced, and a fixed compound remained, which required a red heat for its fusion, and was not altered by the strongest heat that could be given to it in the glass tube; but was rapidly decomposed when heated in an open vessel, muriatic acid fumes being evolved, and oxide of manganese formed, which was black or red, according to the intensity of the heat applied. The compound of manganese and chlorine is a very beautiful substance, it is of great brilliancy, generally of a pure delicate light pink colour, and of a lamellar texture, consisting of broad thin plates.

Mode of freeing it from iron.

There is not much difficulty in obtaining this compound pure. Iron, with which manganese is commonly contaminated, may be separated by two or three repetitions of the solution of the compound in water, the evaporation to dryness of the clear filtered muriat, and fusion of the residue procured by evaporation. Indeed, I think this a good general method for purifying manganese from iron. One of the combinations of the latter metal and chlorine being volatile, heat must separate it from the compound of manganese. And I have thus obtained it so free from iron, that triple prussiate of potash, added to its solution in water, gave merely a white precipitate, without the slightest tint of blue.

Its properties.

This compound deliquesces when exposed to the atmosphere, and is converted into the white muriate. Like ferrane, it affords a trifling residue when heated with water. The residue is oxide of manganese, white at first, but soon becoming red, and even black; it varies in quantity, according to the exclusion of air in the formation of the combination.

50 grains of the compound dissolved in water, with the exception of 1 grain; this residue was separated by decantation of the fluid, washed, dried, and heated to redness, it was in the state of black oxide. The colourless solution was precipitated by nitrate of silver. The horn silver formed, when dried, was equal to 108 grains. Hence, omitting the 1 grain of mixed oxide, 100 of this compound appear to consist of Analysis.

54 chlorine
46 manganese

100

The horn lead, that I have analysed, was made by the decomposition of the nitrate of lead by muriatic acid, and it was well washed, dried, and fused in a glass tube with a small orifice. The strongest red heat that I could apply to it, under these circumstances, did not occasion its sublimation. Horn lead.

50 grains of it, that had been fused, were dissolved in water. This solution, heated with nitrate of silver, afforded 52·65 grains of dry horn silver. Hence 100 of horn lead appear to be composed of Analysis.

25·78 chlorine
74·22 lead

100·00

As this compound, when decomposed by an alkali, affords the protoxide of lead, it may be called plumbane.

The butter of zinc I have examined was obtained by evaporating to dryness the muriate of this metal, and by heating to redness the residue in a glass tube. This compound is not volatile at a strong red heat in a close vessel, it fuses before it acquires a dull red heat, and on cooling it goes through several degrees of consistence, being viscid before it becomes solid. Butter of zinc.

This compound, when heated with water, affords a small residue of oxide of zinc, which, as in the preceding instances, may be considered as in the state of mechanical mixture.

In consequence of its powerful attraction for water, it is a very deliquescent substance; on this account it is necessary to weigh it in water to avoid errour. 49·5 grains of it, thus weighed, dissolved entirely in water, with the exception of 1 grain of oxide of zinc, which was separated by decantation, and dried and ignited, and its quantity ascertained to be as stated. The solution precipitated by nitrate of silver afforded 99 grains of dried horn silver. Hence, excluding the 1 grain of oxide, 100 of butter of zinc seem to consist of

50 chlorine
50 zinc
———
100

This compound may be called zincane.

Fuming liquor of arsenic.

A compound of chlorine and arsenic has been long known, bearing the name of the fuming liquor of arsenic. It may be formed in several ways: by the combustion of arsenic in chlorine gas; by heating in a retort a mixture of arsenic and corrosive sublimate, or of arsenic and calomel; and by the distillation of muriate of arsenic with concentrated sulphuric acid. The old method by means of corrosive sublimate appears best adapted for procuring it in a pure state. About 6 parts of corrosive sublimate to 1 of arsenic are, I find, proper proportions. The mixture of the two substances should be intimate, and the heat applied to the retort for the distillation of the fuming liquor, gentle. When the liquor was not colourless at first, I have purified it by a second distillation.

Its properties.

The fuming liquor of arsenic, it is well known, is decomposed by water. The precipitate produced appears to be merely white oxide of arsenic, for, independent of other circumstances, it does not afford the fuming liquor when heated with strong sulphuric acid.

The fuming liquor, when gently heated, dissolves phosphorus, but it retains on cooling only a very small portion of this substance. The warm solution is not luminous in the dark.

The fuming liquor also, when warm, readily dissolves sulphur; indeed sulphur fused in the liquor seems capable of combining or of mixing with it in all proportions; but on cooling the greatest part of the sulphur is deposited, and assumes a fine crystalline appearance; the form of the crystals was apparently the octahedron. This deposition seems to be merely sulphur with a little of the fuming liquor between the interstices of the crystals, for the crystals bear washing, and become tasteless superficially, but remain still acid internally, where the water has not penetrated.

It likewise dissolves resin. That which was called rosin was the subject of experiment. The solution was of a blueish green colour; but when gently heated it became brown, and remained so on cooling. The portion of resin the fuming liquor is capable of taking up is very considerable; when the resin was added in excess, a viscid mixture was formed. The resinous solution was decomposed by water, and the resin was separated, apparently unaltered, mixed with white arsenic.

The fuming liquor is capable of combining with oil of turpentine and with olive oil. When the mixture was made with either of these oils, there was a considerable elevation of temperature, and a homogeneous colourless fluid was in each instance obtained.

Analogous to the fuming compounds of sulphur and phosphorus.

In these and some other properties, the fuming liquor of arsenic is analogous to the fuming compounds of chlorine and sulphur, and chlorine and phosphorus; these two, having the power of dissolving sulphur, and phosphorus, and resin, and of entering into union with the fixed and volatile oils.

Its component parts.

It is difficult to ascertain the proportion of the constituent parts of this compound by the ordinary modes of analysis, I have chosen therefore a synthetical method in preference; and from repeated experiments I find, that 2 grains of arsenic require for complete conversion into the fuming liquor 4 cubic inches exactly of chlorine gas.

The experiments were thus conducted: the arsenic in one piece was put into a small glass retort having a stop-cock; the retort was exhausted, and a known volume of chlorine gas was admitted from a graduated receiver by means of other stopcocks; and the absorption of chlorine, after the entire conversion of the metal into the fuming liquor, was considered as the proportion condensed by the arsenic.

Now, since 100 cubic inches of chlorine gas weigh just 76·5 grains, 2 grains of arsenic combine with 3·06 grains of chlorine, the weight of 4 cubic inches of the gas. Hence 100 of the fuming liquor appear to consist of

60·48

60·48 chlorine
39·52 arsenic

100·00

As the fuming liquor gives the white oxide when decomposed by water, arsenicane may be substituted for its old name.

Butter of antimony.

The butter of antimony is a well known substance. That which I have examined was obtained by heating together corrosive sublimate and antimony, or antimony and calomel; and was always purified by a second distillation at a low temperature. The best proportion of corrosive sublimate and the metal for making the compound, I have found to be about 2½ parts of the former to 1 part of the latter.

Its properties.

The butter of antimony, like arsenicane, is capable, when rendered fluid by heat, of dissolving resin and sulphur, and of combining with the fixed and volatile oils. It affects the oil of turpentine very like the liquor of Libavius; the action is considerable, much heat is produced, and the oil is rendered brown.

When the butter of antimony is decomposed by a sufficiently large quantity of the hydrosulphuret of potash, that compound is formed, which is commonly called the golden sulphur of antimony; and which, when decomposed by heat, I have found to afford merely water and sulphuret of antimony*.

Its component parts.

To ascertain the proportion of antimony in the butter of antimony, 60·5 grains of this substance, colourless and crystallized, weighed in water, were heated in a solution of hydrosulphuret of potash. The whole of the antimony was dissolved, and the hydrosulphuret of potash being in excess, there was no precipitation on cooling. The solution was decomposed by muriatic

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Golden sulphur of antimony.

* These results appear to me to demonstrate the truth of Mr. Proust's opinion, that the golden sulphur is a hydrosulphuretted oxide of antimony. From my experiments the only difference of composition between kermes mineral and the preceding compound seems to consist in the former containing a smaller proportion of sulphuretted hidrodrogen than the latter; for I have obtained by the decomposition of kermes mineral, by heat, a compound of sulphuret of antimony and protoxide; and I have converted kermes into the golden sulphur, by means of water impregnated with sulphuretted hidrogen.

muriatic acid, and the golden sulphur thus thrown down was collected on a filter, well washed, and dried; heated slowly to redness in a glass tube, steam in plenty was disengaged with very slight traces of sulphur, and sulphuret of antimony remained, which, fused into one mass, weighed 45 grains. According to the experiments of Proust, which I have repeated with the same result, sulphuret of antimony contains 74·1 per cent of metal. Hence 45 grains of sulphuret, or the 60·5 of butter of antimony, from which the sulphuret was procured, must contain 33·35 of metal; and considering the remainder 27·15 of the 60·5 as the proportion of chlorine, 100 of the butter of antimony seem to consist of

39·58 chlorine
60·42 antimony

100·00

This compound, as it yields, when decomposed by water, the submuriated protoxide, may be called antimoniane or stibiane.

Butter of bismuth.

A compound of bismuth and chlorine has been long known, bearing the name of the butter of bismuth. It is obtained both when bismuth is heated with corrosive sublimate and calomel. 2 parts of corrosive sublimate to one part of metal I have found good proportions for its preparation. There is some difficulty in procuring it pure, and entirely free from the mercury revived; this is most readily effected by keeping the butter of bismuth in fusion, at a temperature just below that at which mercury boils; the mercury slowly subsides and collects in the bottom of the vessel, and this operation, continued for an hour or two, affords a pure, or nearly pure, butter of bismuth. Thus prepared, it is of a grayish white colour, opaque, uncrystallized, and of a granular texture. In a glass tube, with a very small orifice, it bears a red heat without subliming.

As a hydrosulphuret of bismuth is produced when the butter of bismuth is heated with the hydrosulphuret of potash; and as this hydrosulphuret, like that of antimony, affords, when decomposed by heat, a sulphuret and water; I have applied the same mode of analysis to this compound as to the last.

Analysis of it.

55 grains of butter of bismuth were decomposed in a warm solution of hydrosulphuret of potash. The dark brown hydrosulphuret of bismuth thus formed, and not dissolved, was

collected on a filter; the hydrosulphuretted solution was decomposed by muriatic acid, the slight precipitate of hydrosulphuret produced was added to the first portion, and the whole was well washed, dried, and heated to redness in a glass tube; the sulphuret of bismuth thus obtained, fused into one mass, weighed 44·7 grains. I had previously ascertained the proportion of metal in this sulphuret, and found it to be 81·8 per cent. 44·7 grains of sulphuret, or 55 grains of the butter, must therefore contain 36·5 grains of bismuth; and hence, 100 of bismuth appear to consist of

33·6 chlorine
66·4 bismuth

100·0

The butter of bismuth may be called bismuthane.

Singularities of these compounds with regard to volatility and fusion.

Among the preceding combinations of the metals and chlorine, there is a surprising difference in respect to volatility and fusibility. Iron and manganese, two difficultly fusible metals, form with chlorine readily fusible compounds, and a combination of the former metal and chlorine is even volatile; the compounds of tin and chlorine, and of chlorine and antimony, are very volatile substances, though the metals themselves are fixed at very high temperatures; on the contrary, the combinations of chlorine with bismuth, zinc, and lead, do not exceed in fusibility; indeed are not quite so fusible as the metals themselves. I can offer no explanation of these phenomena.

Singularity of the fuming compounds.

Another singularity attending the liquid fuming compounds of chlorine, such as the liquor of Libavius, the fuming liquor of arsenic, and the oxymuriates of sulphur and phosphorus, is, that they do not become solid at low temperatures. I have reduced, by means of a mixture of snow and muriate of lime, the temperature of all these substances 20 degrees below the zero of Fahrenheit's thermometer, but without affecting their liquidity.

Influence of atmospheric air at high temperatures.

The influence of atmospheric air on the compounds of the metals and chlorine at high temperatures is curious, and worthy of particular attention. The combinations of chlorine with lead, zinc, copper, and bismuth, appear to be volatile in open vessels, and fixed in closed ones. How moist air operates in these instances, it is difficult to say. In other cases, where it evidently acts chemically, the changes explain themselves; thus, when the compounds of iron and chlorine, and of manganese and chlorine, are heated in the open air, hygrometrical water of the atmosphere seems to be decomposed, as muriatic acid fumes are produced, and oxides of the metals formed. Probably the volatility of the other compounds is connected with similar circumstances. This action of moist air has hitherto been much neglected; it is certainly worthy of being more fully inquired into, both in a theoretical and practical point of view. Its importance in practice is exemplified in the reduction of horn silver, and in the formation of several of the compounds of chlorine and the metals; if moist air be admitted in these operations, the silver will be lost, and the compounds not formed.

Action of moist air important.

Action of heat without air on muriate of magnesia.

Guided by analogy, I have been led to try whether the muriate of magnesia, which is readily decomposed by heat in the open air, would not, when the air was excluded, by introducing it into a glass tube with a very small orifice, afford a permanent compound. The result was agreeable to my expectations; I obtained, by strongly heating the muriate for a quarter of an hour, a substance like enamel in appearance, being semifused, and which appeared to be a mixture of magnesia and the true compound of magnesium and chlorine, for heated with water magnesia was separated, and a muriate of magnesia formed.

5. *On the Relation between the Proportion of Oxigen and Chlorine in Combination with several Metals.*

Doctrine of definite proportions a test of the accuracy of analyses.

Errours being very common in chemical analyses, even in those conducted most skilfully and carefully, all possible means should be taken to discover them; and no means, I think, promise to be more effectual for this purpose, than the general analogy of definite proportions. From a great variety of facts, it appears that oxigen and chlorine combine with bodies in the ratio of 7·5 to 33·6. With 1 part by weight of hidrogen, for example, 7·5 of oxigen unite to form water; and 33·6 of chlorine unite with the same proportion to produce muriatic acid gas. To judge therefore of the accuracy of the analyses of the preceding combinations of the metals and chlorine, it is only necessary to compare them with the analyses of the oxides of the same metals. If the two agree, there will be reason to consider them both correct, but should they disagree, there is equal reason for supposing one or both of them to be wrong.

Applied to the compounds of copper;

Thus as the orange oxide of copper is analogous to cuprane, and the brown oxide to cupranea, the oxigen and chlorine should be to each other in these compounds as 7·5 to 33·6. And from comparison of my analysis with those of Mr. Chenevix and Mr. Proust, it appears, that in the two first, copper being as 60, the oxigen is to the chlorine as 7·79, instead of 7·5 to 33·77, instead of 3·36; and in the two last as 7·5 to 33·6, or as 15 to 67·2. Coincidences as near as might be reasonably expected.

of tin;

There is not the same agreement between Mr. Proust's analyses of the oxides of tin and the preceding ones of the combinations of this metal and chlorine. This discordance induced me to repeat my analyses; and, obtaining the same result as at first, I directed my attention to the oxides of tin, and made the following experiments to ascertain the proportion of their constituent parts.

(Experiments on the oxides of tin.)

42·5 grains of tin, which had been precipitated from the muriate of this metal by zinc, were heated with nitric acid in a platina crucible, and slowly converted into peroxide; the acid and water were driven off by gentle evaporation at first, and afterward by a strong red heat, continued for a quarter of an hour. The peroxide thus produced was of a light yellow colour, and being very gradually dried, it was semitransparent, and hard enough to scratch glass; it weighed 54·25 grains. Hence, as 42·5 grains of tin acquire, on conversion into peroxide, 11·75 grains of oxigen, this oxide appears to contain 21·66 per cent of oxigen, just the quantity found in the native oxide by Klaproth, instead of 28, the proportion stated by Proust.

Mr. Berthollet, jun., has shown, that Mr. Proust's estimate of 20 per cent of oxigen in the protoxide is incorrect. To ascertain the true proportion, 20 grains of tin were dissolved in strong muriatic acid in a retort connected with a pneumatic apparatus, and without the assistance of heat; 16 cubic inches of hidrogen gas were produced. (Barom. 30, thermom. 60.)

As the production of this quantity of hidrogen indicates an absorption of oxigen by the tin equivalent to 8 cubic inches, or (as 100 cubic inches weigh 34·2 grains) to 2·736 grains, the protoxide of tin appears to contain 11·99 per cent of oxigen.

These analyses of the oxides, compared with those of the combinations of tin and chlorine, are found very nearly to agree. The ratio of oxigen to chlorine in the first two similar compounds, the tin being as 55, is as 7·5 to 33·4; and in the last two, viz. the peroxide and the liquor of Libavius, as 7·6 to 33·5, or as 16·2 to 67.

As the black oxide of iron is formed by the decomposition of ferrane by a solution of potash, and the red oxide by that of ferranea, it is evident, that these oxides and combinations of iron and chlorine should coincide in the proportions of their constituent parts. This appears from the analyses* of Dr. Thompson to be nearly the case; for, iron being as 29·5, the oxigen is to the chlorine in the black oxide and ferrane as 8 instead of 7·5 to 33·6; and in the two others as 8 to 33·6, or as 13·2 to 55·5. Here the agreement is less than in other instances; but this is not surprising, considering the different estimates of the proportions of oxigen in the oxides of iron, and the difficulty of ascertaining them correctly. of iron;

The yellow oxide of lead, and the white oxides of antimony, bismuth, zinc, and arsenic, are formed, when the combinations of these metals and chlorine are decomposed by a solution of potash. But on comparison with the best analyses of the oxides, there is not, excepting in the case of zinc and arsenic, that coincidence of proportions which might be expected. Zinc being as 34·5, the oxigen in the oxide, from the analysis of Proust, is to the chlorine as 7·5 to 34·4; and the arsenic being as 21·9, the oxigen, from the analysis of the same chemist, is to the chlorine as 7·3 to 33·6. The analyses of the oxides of the other metals being at variance with those of the chlorine combinations, I was induced to make the following experiments, with the hope of discovering the cause of the difference. of other metals; of zinc; of arsenic;

100 grains of lead, which had been precipitated from the nitrate of lead by zinc, were dissolved in nitric acid, and thrown down by carbonate of potash. This precipitate of carbonate of lead of lead;

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* Nicholson's Journal, vol. XXXVII, p. 375.

lead was well washed and dried, and heated to dull redness for a quarter of an hour in a platina crucible: by this treatment all the carbonic acid was expelled; the remaining yellow oxide weighed 107·7 grains, and it dissolved in muriatic acid without effervescing, and without affording any residue of brown oxide. Hence, the yellow oxide of lead appears to contain 7·15 per cent of oxigen. And this proportion of oxigen in the oxide compared with that of chlorine in plumbane, lead being as 97·2, appears to be in the ratio of 7·5 to 33·8, instead of that of 15·6 the estimate of Klaproth, or of 11·2 the estimate of Dr. Thompson to 33·8. Klaproth might have been misled by considering the hydrated oxide as a true white oxide free from water.

(Oxide of lead)

of antimony;

(Oxides of antimony.)

According to Mr. Proust, the peroxide of antimony contains 23 per cent of oxigen, and the protoxide 18*. I have repeated this chemist's experiments; my results, in which the peroxide is concerned, agree with his; but there is not the same concordance in those relating to the protoxide. The protoxide I used was either prepared by the decomposition of the butter of antimony, or of the sulphate, by a boiling solution of carbonate of potash. This oxide, in its purest state, I have always found, as Mr. Proust describes it, of a light fawn colour before fusion, and afterward in mass of a gray colour, and of a radiated crystalline texture. 100 grains of it that had been fused were heated in the state of powder with strong test nitric acid in a platina crucible: when nitrous gas ceased to be produced, the excess of nitric acid was expelled by a gentle heat, and the oxide was heated to dull redness; the increase of weight after this was equal to 10·4 grains: nitric acid was again added, and the process repeated, but without any alteration of weight being produced. Hence, as the peroxide contains 23 per cent, the protoxide seems to contain 15 per cent; which proportion of oxigen very nearly agrees with that of chlorine in the butter of antimony; for, antimony being as 42·5, the former is to the latter as 7·5 to 34·6, instead of 33·6. I put some confidence in this estimate of the proportion of oxigen in the protoxide, not only on account of its agreement with the analysis of the butter of antimony, but because it was confirmed on the repetition of the experiment.

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* Journal de Physique, Tom. LV,

Klaproth concludes from his experiments, that the oxide of bismuth, prepared by means of nitric acid, contains 17·7 per cent of oxigen; and in consequence this oxide has been considered distinct from that which is formed by direct calcination of the metal, and which contains a much smaller proportion. But there is reason to believe, that this difference does not really exist, and that there is only one known oxide of bismuth, and that Klaproth's oxide was a hydrated oxide; for I have found that 100 grains of bismuth, converted by nitric acid into oxide, precisely in the same manner as the protoxide of antimony was more highly oxidated, gained only 11·1 grains. Klaproth did not heat his oxide to redness, and hence apparently the discordance. From the above result, which I have confirmed by repetition of the experiment, oxide of bismuth seems to contain 10 per cent of oxigen; and bismuth being as 67·5, the oxigen in the oxide is to the chlorine in the butter of bismuth, as 7·5 to 34·2.

and of bismuth.

(Oxide of bismuth.)

6. *On the Relation between the Proportion of Sulphur in the Sulphurets, and the Proportions of Chlorine in some of the Combinations of Chlorine and the Metals.*

The last section afforded proofs of the useful application of the general analogy of definite proportions in correcting the results of chemical analyses. In the present section, it is my intention to pursue a little farther the plan that I have adopted in the preceding, and to apply another test to the analyses of the combinations of the metals and chlorine, by comparing some of them with the combinations of the same metals and sulphur.

Proportions of sulphurets compared with those of the compounds of oximuriatic acid.

I was first led to examine the sulphurets of tin on a different account. Aurum musivum, it has been observed, is formed when stannane is heated with sulphur. According to Mr. Proust, this substance is a sulphuretted oxide of tin. Were this opinion correct, an argument might evidently be deduced from it in favour of the existence of oxigen in chlorine. To satisfy myself respecting this, I endeavoured to ascertain whether any sulphureous acid gas is produced by the decomposition of aurum musivum by heat, as it is commonly asserted. I heated to redness in a bent luted green glass tube, connected with a pneumatic mercurial apparatus, about 20 grains of aurum musivum, prepared by the decomposition of stannane

Examination of aurum musivum.

with sulphur, no more gas was produced than the expansion by heat occasioned, sulphur sublimed, and a gray sulphuret of tin remained. These results I have several times obtained, and not only with aurum musivum prepared as the preceding, but with some also made according to WOULFE's process. As no sulphureous acid gas was produced, and as sulphur sublimed, it may be concluded, that aurum musivum differs merely from the gray sulphuret in containing a larger quantity of sulphur. My next object was to ascertain the exact proportion of sulphur in both these sulphurets, for the sake of comparison with the combinations of tin and chlorine.

Component parts of the gray sulphuret of tin,

100 grains of tin in a finely-divided state, as precipitated from the muriate of this metal by zinc, were heated in a glass tube intimately mixed with sulphur, the combination of the two was accompanied with vivid ignition, the sulphuret formed weighed 127·3 grains, and, broken, it appeared perfectly homogeneous; it was pounded, and again heated with sulphur; but the excess of sulphur being expelled, the fused sulphuret had not increased in weight. The second time I made this experiment, I obtained the same result.

and of aurum musivum.

50 grains of aurum musivum, purified from mixed sulphur by exposure in a close vessel to a dull red heat, were decomposed by a bright red heat in a small green glass tube nicely weighed, and having only a very small orifice; the loss of sulphur, by conversion into the gray sulphuret, was equal to 9·3 grains. Hence, as 40·7 grains of gray sulphuret contain 8·72 grains of sulphur, 50 grains of aurum musivum appear to contain 18·02 grains.

Ratio in which sulphur combines with bodies, compared with oxigen, and oximuriatic acid.

The ratio in which sulphur combines with bodies is to that in which oxigen and in which chlorine combine, as 15 to 7·5 and 33·6. This appears from the proportions of the constituent parts of sulphuretted hidrogen and sulphureous acid gas; for I have found 100 cubic inches of the former to weigh 36·64 grains, and 100 of the latter 68·44 grains. In the comparison, therefore, between the sulphurets of tin and the combinations of this metal and chlorine, 15 by weight of sulphur are equivalent to 33·6 of chlorine. And the tin being as 55, it appears from the analysis of the gray sulphuret and stannane, that the sulphur is to the chlorine as 15 exactly to 33·4; and from the analysis of the other two compounds, aurum musivum, and the liquor of Libavius, as 15·5 to 33·5, or as 31 to 67.

Sulphurets of iron,

The proportions of sulphur in the two sulphurets of iron do not accord with the proportions of oxigen in the oxides, or of chlorine in the chlorine combinations; but I am yet ignorant of the cause of this difference.

of lead,

100 grains of lead, heated with sulphur in a glass tube, afforded, in two trials, 115·5 grains of fused sulphuret. Hence, lead being as 97·2, the sulphur is to the chlorine in the respective combinations as 15·09 to 33·8.

of antimony,

Sulphuret of antimony contains 25·9 per cent of sulphur. Hence, antimony being as 42·5, the sulphur in the sulphuret is to the chlorine in the butter of antimony, as 14·86 to 34·6.

and of bismuth.

100 grains of bismuth heated with sulphur afforded 122·3 grains of sulphuret. Hence, bismuth being as 67·5, the sulphur is to the chlorine as 15·08 to 34·2.

In the following table, the proportions are collected in which chlorine, sulphur, and oxigen combine with several metals; the numbers representing the metals are kept constantly the same, for the greater facility of comparison.

Compounds of metals with oximuriatic gas, oxigen, and sulphur.

Copper	60	+ 32·77 chlorine	= cuprane.
		+ 67·20 ditto	= cupranea.
		+ 7·79 oxigen	= orange oxide.
		+ 15·00 ditto	= brown oxide.
Tin	55	+ 33·40 chlorine	= stannane.
		+ 67·00 ditto	= stannanea.
		+ 15·00 sulphur	= gray sulphuret.
		+ 31·00 ditto	= aurum musivum.
		+ 7·50 oxigen	= protoxide.
		+ 15·20 ditto	= peroxide.
Iron	29·5	+ 33·60 chlorine	= ferrane.
		+ 55·50 ditto	= ferranea.
		+ 8·00 oxigen	= black oxide.
		+ 13·20 ditto	= red oxide.
Manganese	28·4	+ 33·60 chlorine.	
Lead	97·2	+ 33·80 chlorine	= plumbane.
		+ 15·09 sulphur	= sulphuret.
		+ 7·50 oxigen	= yellow oxide.
Zinc	34·5	+ 34·40 chlorine	= zincane.
		+ 7·50 oxigen	= oxide.

Arsenic 21·9 + 33·60 chlorine = arsenicane.
+ 7·30 oxigen = white oxide.
Antimony 42·5 + 34·60 chlorine = antimonane.
+ 14·86 sulphur = sulphuret.
+ 7·50 oxigen = protoxide.
Bismuth 67·5 + 34·20 chlorine = bismuthane.
+ 15·08 sulphur = sulphuret.
+ 7·50 oxigen = oxide.

7. *On the Action of muriatic Acid on some Combinations of Chlorine and Metals.*

Action of muriatic acid on some compounds of oximuriatic acid and metals.

Sir Humphry Davy has pointed out in a great variety of instances the existence of an analogy between chlorine and oxigen. He has shown, that the former, united with certain inflammables, constitutes, like the latter, acid compounds; and combined with metals, as it has already been observed, substances similar in many respects to metallic oxides.

I have kept this analogy in view in my inquiries; and, directed by it in my experiments, I have obtained some results which appear to me to coincide with it.

Thus having been led to try the action of muriatic acid on different combinations of the metals and chlorine, I have found many of them capable of uniting with this acid, and of forming compounds not dissimilar to some of those consisting of acids and metallic oxides.

Corrosive sublimate, stannane, cuprane, and the combinations of chlorine with antimony, zinc, lead, and silver are all soluble in different degrees in muriatic acid.

Corrosive sublimate.

Corrosive sublimate, which is but sparingly soluble in water, and still more sparingly in the sulphuric and nitric acids, is, I have ascertained, very readily soluble in muriatic acid. 1 cubic inch of the common strong acid takes up about 150 grains of this substance, and when gently heated, a quantity far more considerable, about 1000 grains. The compound thus formed solidifies on cooling into a crystalline fibrous mass, of a pearly and brilliant lustre. It is decomposed by heat, the acid being first expelled; and when exposed to the atmosphere, it effloresces, and appears to lose its acid; for, afterward analysed, it is found to be pure corrosive sublimate.

Whe

Compounds of silver, of copper, and of lead.

When I first tried the action of muriatic acid on the different combinations of chlorine already mentioned, I was not aware, that KLAPROTH had before observed the solubility of horn silver in this acid, and Mr. CHENEVIX that of cuprane. Horn silver, cuprane, and horn lead, are precipitated from muriatic acid unaltered by water. Both the hot saturated solutions of the two last compounds deposit crystals on cooling; those from the solution of the former are of an olive green colour, and of a prismatic form, and consist of cuprane and muriatic acid; those from the latter are small white brilliant plates.

None of the compounds absorb muriatic acid gas.

Finding the combinations of the metals and chlorine so generally soluble in liquid muriatic acid, I expected, that some of them might absorb muriatic acid gas; but none that I have tried have possessed this property, not even the liquor of Libavius. Indeed this is not singular, for water is necessary to the composition of many saline bodies; neutral carbonate of ammonia and nitrate of ammonia, for instance, cannot be formed without the presence of water. Neither is the precipitation of cuprane, horn silver, and horn lead from muriatic acid by water extraordinary; there are several salts containing metallic oxides which are liable to the same change, the oxides having less affinity for the acid, than water has.

Precipitation by water not extraordinary.

The action of muriatic acid may explain many phenomena.

The action of muriatic acid on the combinations of the different metals and chlorine will, I have little doubt, afford, when more minutely investigated, explanations of many phenomena, which are not yet well accounted for. Before I conclude, I shall mention only one instance, to which it already appears to be applicable. Mr. PROUST has observed the decomposition of calomel by boiling muriatic acid, and its conversion into corrosive sublimate and running mercury. Now calomel being insoluble in muriatic acid, these changes evidently appear to be owing to the strong attraction of the acid for corrosive sublimate, which has been already shown to exist.

Decomposition of calomel by it.

VIII.

VIII.

On the Coral Fishery in the Sicilian Seas: *by* ALFIO FERRARA, *M. D.*

Mylarum at pontus, Drepanique, et stricta Pelori
Claustra ferunt avidis ramosa corallia nautis.
Flaccomius in Sicelid.

Communicated by the Author.

Coral a subject of natural history worthy notice.

The author's study of it.

HAVING for a long time employed myself in the study of the various natural productions, with which the sea that bathes the Sicilian shores abounds, the coral was the first object to attract my notice. This beautiful and elegant ornament of the sea could not fail of deserving first to come under my examination. I have been frequently present at the fishing of it, near the coast of Sicily: I have contemplated it in the very bottom of the *sea*, on its native spot: I have gathered it from stones, and shells, and other marine substances, recently taken out of the sea: I have had it worked in my presence: I have analysed the several varieties of it: in fine, I have extended my researches to whatever would give me the least insight into the nature of this substance, comparing the results of my own observations with every thing the ancients and moderns have written on the subject, and consulting in every point the treasures of natural history, with which the present day has been so abundantly enriched by the accurate experiments and luminous theories of the many great men of the last century.

Object of the present paper.

I have endeavoured in the present memoir to establish a clear and precise notion of the origin, increase, and nature of coral. This work has been the more pleasing to me, as I flatter myself I have been able not only to confirm by my own observations what has been already written on the subject by former Philosophers and Naturalists, but to add some new facts, that may tend to elucidate the history of this marine production, which has at all times as much occupied the researches of naturalists, as it has engaged the admiration of the fair sex, with whom the beauty of its colour, and brilliancy of its texture, have rendered it a favourite ornament of dress.

Supposed to be a plant by the ancients,

The ancients, attending only to its external form, conceived coral to be a plant; to which from its ramifications it bears some resemblance, and named it lithodendron, or stony plant, on account of its hardness. It was so called by Dioscorides and Pliny. These authors and their contemporaries did not attempt to contradict by the most trifling examination, what the poet Ovid (his head full of transformations) had asserted: that under the water it was a soft plant, but, immediately on being taken from the sea, became hard. This opinion prevailed for a long time, and was encouraged in later times by many great naturalists. Of this number was the celebrated Cesalpino.

and many moderns.

Baccone's opinion of it.

Our Baccone, who took much pains to investigate the nature of coral, could not divest himself of this idea; but, gifted as he was with great sagacity and penetration, not being convinced, either from his own observations or those of others, that coral was a mere plant, and still less that it was a stone, he imagined, that the milky juice, which drops from the pores of fresh coral, was its seed; which, being dispersed in the sea, is precipitated and gradually accumulated in a regular form in the capsules nature provides for it*.

Count Marsilli's.

This opinion, tending to alienate naturalists from the belief of the vegetable nature of coral, was entirely removed by the publication of the valuable and erudite work of the celebrated conte Marsilli, entitled Storia del Mare; who, led away by his imagination, or rather deriving little aid from the state of natural philosophy at that time, suggested the idea, that the movable substances at the extremity of the branches were the octopetalous flowers of the coral, and thus revived the old opinion.

Embraced by Tournefort and others,

Tournefort, who, in the pursuit of his favourite study of botany, had remarked the vegetation of stones in the grotto of Antiparos, eagerly adopted this idea; and was followed by Ray, Boerhaave, Klein, and many others of that time.

Supposed to be a plant encrusted by calcareous earth.

No sooner had naturalists begun again to take up the observations of Baccone, than they discovered in the hard substance of coral a sort of earthy concretion: but this not being sufficient to induce them to expunge it from the list of vegetable substances, they considered it as a marine plant encrusted with calcareous earth deposited by the sea. Lehman was of this opinion, to which the mineralogist Baumer was also much inclined.

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[*] See Rechescher sur le Corail, and Museo di Fisica.

Ferrante Imperato first supposed it to be the habitation of worms.

This opinion generally adopted.

Our Ferrante Imperato, in his work on natural history (which, like many other works of the ancients, has been almost buried in oblivion, though well deserving our attention from its containing the principles of many important truths, which have since been brought to light), had already supposed, that some of the species of coral were merely the habitation of marine worms. This opinion had so much of probability, that it has always been entertained by naturalists since; and the discovery of the polypi assists to explain on solid principles the true nature and origin of coral: and on this account the works of Peyssonnel, Jussieu, Guetard, Trembley, Reaumur, Donati, Ellis, Pallas, Cavolini, Spallanzani, and many others on coral, became so interesting. Coral is found round nearly all the Mediterranean islands. Pliny and Dioscorides speak much in praise of that found in the Sicilian seas* in their time. It is fished for at present on every part of the shores of Sicily.

Places where the coral is collected.

Requires eight years for its reproduction.

The Messineze collect a great quantity in those straits, even as far as Melazzo; but the Trapanese, who are chiefly employed in working the coral, not only fish it in the neighbouring seas about the Eolian and other islands, but extend their search to all the Southern shores as far as Cape Passaro, and beyond Siracuse, and even to the coast of Barbary. They are obliged to occupy so large an extent of sea; as they cannot fish again on the same spot for several years, the re-production of coral requiring a great length of time, even nearly eight years. I have myself collected it on the shores of Catania, and thence as far as Taormina.

Instrument used to get it.

The instrument with which the coral is detached from the bottom of the sea has been known a long time. It is composed of a large wooden cross, having fastened to each of its four extremities nets sufficiently capacious to enclose the coral, which is broken from its root by a large stone hanging from the centre of the cross. The instrument is let down by two ropes from the boats employed in this fishery into the sea, and after remaining a sufficient time it is drawn up by a windlass. The Trapanese claim the invention of this machine.

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* See Dioscorides, lib. 5; Plin. lib. 32. Pliny says, *Laudatissimum in Gallico sinu circa Stoechades insulas, et in Siculo circa Heliam, ac Drepanum.* Some commentators, not finding the name of Helia, have called it Aeolias; but the true name is Helia, for the island opposite Trapani was anciently so called. Pliny himself names it in his 3d *book Hieronesus.*

Coral adheres to some hard substance.

From my own observations, and from the most accurate information I have been able to obtain from the people employed in this fishery, I am persuaded, that the coral grows indiscriminately on all hard substances, as rocks, shells, &c.—I have seen it attached to an earthen vessel, which had at some time fallen into the sea, and was taken out in my presence. The usual appearance of coral is that of a tree without leaves. It never grows to a greater height than twelve inches, and is seldom an inch thick. The direction of its branches extends always forwards from the spot to which the root is attached; therefore when it grows on the top of a cavern they spread downwards; if from a horizontal surface upwards: most commonly however the branches extend downwards, which enables the nets to enclose it with greater facility when detached by the stone.

Its figure and size.

Branches broken off fix on some hard substance.

It has been constantly remarked, that the broken branches of coral attach themselves to some hard substances where they continue their growth. It is very common to find many branches of coral, when taken out of the sea, perforated in several parts. There can be no doubt, that this is the work of the lithophagi; worms which attack even the hardest substances, for it is well known that they pierce and destroy the hardest carbonate of lime. The coral (isis nobilis, Linnei) which is most eagerly sought after, is of a fine red colour. Artists and ladies give it the preference. It improves the charms of a beautiful face. Naturalists describe all the varieties; two original colours in coral may be established, white and red, as the two extremes, the gradations of shade from the one to the other producing infinite varieties, among which five principal may be distinguished.

They are frequently perforated by worms.

Red most esteemed.

Varieties.

1st. The deep red coral resembling in colour minium. This is considered as the most perfect sort; in fact, it is the largest and most dense, and receives the highest polish. It is commonly called the male coral.

2d. Red coral. This is more or less clear, but always less brilliant than the first variety.

3d. Flesh coloured coral. The ancients call it light red.

4th.

Varieties.

4th. Dull white coral; by some it is called fawn coloured, from its resemblance to the colour of the fawn.

5th Clear white coral. All these varieties are found in the seas round the island, sometimes on the same spot. The first and second are not so abundant or common as the others.

Appearance of coral when fished up.

The extremities of coral, when extracted from the sea, are swelled and rounded, resembling juniper berries. Probably these were the berries remarked by Pliny, which he considered as the fruit of the coral; although in his work he asserts that they are white and soft under water, and become hard and red out of it. I am inclined to believe, either, that he wrote from the reports of others, or that he has mistaken for them the red globules formed by the artist. These extremities when pressed, give out a white unctuous fluid resembling milk, which has a sour taste. It was formerly thought to be the seed and nutritious juice of the coral plant.

Fluid expressed from it.

Coral hard in the sea, and red

The substance of coral is hard as well in the sea, as when out. The red kind is red from the first, and it is a singular circumstance, that the ancients should have entertained these two erroneous opinions, which the most simple examination would have falsified.—The central part or axis of the coral is hard, of a firm solid texture, even, and lamellated; and hence capable of taking the finest polish. This is enclosed by a paler coloured bark of a granulated texture, interspersed with holes in the form of stars with eight rays. In the coral of the largest size sometimes is found a kind of joint or union between the different pieces of which it is composed, these having the appearance of tubes of some length, lying one above the other. In the analysis of coral we obtain a small quantity of gelatinous animal matter, a large proportion of carbonate of lime, and a little iron. The different colours of this beautiful marine production seem to depend on the different degrees of oxidation of the iron, and various proportions of it in union with the animal matter. The discovery of polypi gave the clearest idea of the origin and growth of coral. These animals, the last in the scale of animated nature, form for themselves small nests sufficiently solid to shelter and protect them. These soft and delicate animals, surrounded by an element in a constant state of agitation, and exposed to the attacks of their numerous enemies, were instructed by nature to form for themselves a covering capable

The centre hardest.

Cortical part.

The largest pieces have an appearance of joints.

Its component parts.

The colour apparently owing to iron.

Coral formed by polypi.

capable of resisting the percussion of the sea, and affording them a retreat in the moment of danger.

The polypi described.

These coralligenous polypi are only a few lines in length, their bodies elongate and ramify into eight delicate threadlike branches around the mouth. These are the arms and legs of the animal, which it can extend and spread out at will to a considerable distance in search of its food. They are analogous to the horns of the snail. The curious manner of propagation of polypi, so different from that of other larger and more perfect animals, is well known; on examining minutely the gelatinous bodies of these polypi, a great number of grains, or little buds, are discernible, covering the surface; these elongate themselves, increase in thickness, diverge and spread in all directions, and become young polypi. Scarcely are these developed before a new series of sprouts appears from their small bodies by the increase and growth of the small buds on their surface. By this rapid succession the family is propagated in every direction, forming as it were a genealogical tree of existing generations. It is well known how from the soft nature of their bodies these animals are enabled to unite and engraft with each other in the same manner as plants; and one branch of these animalculæ so engrafted lives and regenerates another. Even one single animal may detach itself from the family tree, and establish on another spot a new family with its various branches. While large animals have bones for the support of the softer parts, and shell fish are protected by their shells, the coralligenous polypi make use of a certain proportion of earth to incorporate with and give firmness to their form.

Growth of the polypi.

Immediately as a polypus has fixed itself on a hard body, it begins to lay the foundation of its future generation. If you only take some stones from the bottom of the sea round Sicily, you will find on them small branches of red coral, and round red spots, which are the first depositions of the coralligenous polypi. In the same way as the bones of the larger animals are formed by the gradual deposition of the earthy particles separated from their food by vessels adapted to this purpose, so is the covering of these polypi formed by the carbonate of lime mixing and encrustating with the gelatinous matter, which is so abundantly secreted by their delicate bodies, and gradually incases them except the mouth. If a branch of coral newly gathered is immersed in a vessel full of sea water, these animals are perceived issuing from the stellated holes, their mouths gradually appearing first, and then their silklike arms extend, in this manner putting on the appearance of octopetalous flowers, by which the ingenious count Marsilli was deceived.

Black coral of the ancients.

The multiplication of polypi, of which I have treated, explains admirably the arboraceous form of coral, as also the increase of the branches detached from the trunk. I have before me a fine specimen of the *antiphates*, the black coral of the ancients, in which the extremity of a branch has united with the principal trunk, and the polypi are seen bedded in it.

Classification of coral.

From what we have seen, I think the term zoophyte inapplicable to coral; it is neither an animal plant, nor a plant animal: Nor can it be called a zoolite; as it is certainly not a stony animal. It is with more propriety a *polipaio*; which, on account of its form, and to distinguish it from the other analogous works of polypi, might be callèd *polipaio dendroide.* In using this nomenclature, we must be careful not to adopt the false idea, that the *polipaio* resembles a wasp's nest; the wasps may at will leave their nest, but the *polipaio* is a part of the animal, from which it cannot be detached. Thus the *polipaio dendroide* is an accumulation of ramified polypi, incorporated with the solid substance, in the same manner as the shell of some animals and the bones of others. The above erroneous opinion cannot be entertained by any one, who observes, that in coral the gelatinous membrane of the polypus is continued into the solid earthy part, the same as in bones. Herissant has already pointed out this mistake.

Its hardness supposed to be progressive,

but this not confirmed by experience.

It is to be inferred from the analogy of coral with bone, that, as it does not arrive at once at a state of maturity, but by degrees, its hardness must also be progressive. However probable this idea may be, it has not been confirmed by experience. I have particularly remarked the small quantity of iron obtained in the analysis of the red coral, I have always found it combined with the gelatinous animal substance in the state of oxide. Not to extend this paper too much, I shall omit the results of various experiments I have made; but they have led me to conclude, that the ferruginous substance is phosphate of iron, that is, the oxide of iron united with phosphoric

The colour of coral ascribed to phosphate of iron.

phoric acid, which it is well known gives the red colour to the blood of animals[*]. The phosphate of iron therefore, which in animals has the property of giving the lively red colour to the blood, and even the vermilion hue to the skin, serves to colour the solid part of coral, and give it the brilliant sanguineous tinge.

Attempt to account for the difference of texture and colours.

The first variety, as I have remarked, is esteemed the most perfect; it is more solid than the other kinds, of a finer and more compact texture, and hence takes a higher polish. In the other kinds, in proportion as the bright colour fades, these qualities gradually decrease, so that the white sort, which is the softest and lightest, is very unfit to be wrought, and takes but a trifling polish. The deficiency in the quantity of phosphate of iron diminishes the colour, and at the same time decreases the density of its texture; or perhaps the light texture by its porosity permits the water to wash away the colouring matter, and consequently that which would tend to bring it to perfect maturity.

The red parts always most solid.

To this may be attributed the peculiarities of some corals, in which the trunk is red, and the branches white; or the branches red within, and externally white; or the branches half white and half red, which is often seen in coralligenous productions; but the red part always proves of firmer texture than the others.

Coral as an ornament

in some respects superior to gems.

While naturalists have been employed in investigating the origin of coral, and the nature of its growth, each applying it to different purposes; the fair sex, occupied by the natural desire of pleasing, have been much indebted to the brilliant colour and fine lustre of this marine production. Coral formed into beads is worn as an ornament of the neck and arms; and there is no doubt, that the lively colour of coral gives additional grace to a fine face and beautiful complexion, which cannot be obtained by the use of the precious stones, so that these can only be considered as ornaments of luxury and show. The ladies who are always led away by fashion, because they consider it as depending on the existing taste of the other sex, laid aside this beautiful ornament, to load themselves with jewels brought from distant countries. Thus coral gave place to other ornaments, the rage of pleasing being only gratified by variety. Works of Amber have latterly obtained a very high estimation from the softness of its substance and its transparency*.

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[*] This is at least highly questionable. See Journal, p. 31, of the present volume, and p. 48 of the preceding. C.

Amber lately become fashionable.

This substance, which for a time was in high repute, and which the discovery of the precious stones had almost thrown into oblivion, has of late, by the accustomed versatility of capricious fashion, recovered its former value, and has rivalled in price even the ornaments composed of jewels.

Coral superior.

When the value of female ornaments shall depend no longer on the price or scarcity, but on the effect they produce on the complexion, all will yield to the natural beauty of coral. Most certainly Galatea, emerging from the ocean, would select from the numerous offerings of the nymphs the lucid branches of the coral to adorn herself with, which would alone assimilate with the roundness of her lips, and with the vermilion of her cheeks.

Method of working coral.

The working of coral consists in removing the outer bark, and exposing the interior solid and highly coloured part, which takes a fine polish. The coarse part of the bark being removed by the file, it is rubbed with tripoli powder, and lastly, with a metallic earth, which gives the polish. Some bring it to the finest polish imaginable by the use of the oxide of tin.

Uses to which it has been applied.

Superstitions respecting it.

The ancients ornamented their swords, bucklers, and helmets with coral; this custom is still in vogue in some part of Asia, where coral is as much esteemed as in the time of Pliny. The soothsayers and priests of that age attributed many mystic properties to it; hence they were in the habit of wearing coral, as well from religious motives, as from regard to its beauty. Paracelsus recommends it to be worn round the necks of infants, as an admirable preservative against fits†, sorcery, charms, and even against poison. Many other follies of that man are still prevalent, and of great credit with the common people; and it is very usual in the inland parts of Sicily, to see

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[*] See Memor. sull Ambra di Sicilia, 8vo, Pal. 1801. Written by my brother, ab Francesco Ferrara, profes. in the univers. of Catania.

[†] The negroes in the West Indies say, that the colour of coral is affected by the state of health of the wearer, it becoming paler in disease.

children wearing amulets of coral round the neck for the above purpose. In the cities it is worn by many in the shape of a horn, as a protection against the influence of evil eyes. It was even believed, that coral would drive away devils and evil spirits, hence perhaps arose the custom of making crowns of it. Nor have the medicinal properties of coral been less exaggerated, as may be sufficiently seen in the writings of Pliny and Dioscorides. It certainly may be considered as an absorbent, it is used in dentifrice powder, in the Alkermes for indigestion, and in the Troches of Carabe.

First wrought by the Trapanese.

The Trapanese appear to have been the first who worked the coral, being induced thereto by the great quantity of it found in their seas. It is asserted, that Antonio Ciminello, a Trapanese, was the first who discovered the art of engraving coral*. In the time of king Alphonso the coral fishery was so assiduously, and so advantageously pursued by the Trapanese, that the ministers of that king proposed to subject the fishery to a tax†. In the last century, when it was again proposed, instead of a tax, which probably would have ruined this branch of industry, king Ferdinand instituted some very useful regulations in favour of it.

Engraved.

Beside forming necklaces and bracelets, the Trapanese have the art of engraving it in the same manner as they do amber and shells, and most certainly many of these works display great spirit, boldness, and grace in the execution, talents natural to the genius of the Sicilian nation.

IX.

On the medical Effects of the Bark of the Piscidia Erythryna of Linnæus, or Jamaica Dogwood. In a letter from WILLIAM HAMILTON, Esq.

To Mr. Nicholson.

Nevis, the 20th of July, 1812.

SIR,

Bark of the root of Jamaica Dogwood used for poisoning fish.

YOUR readers may perhaps have accidentally heard of the remarkable effects produced upon fish, by mixing a strong publication; and, I believe, you will agree with me in the opinion, that they must entirely set the question at rest.

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* Orlandini Descrizione di Trapani.

† Capit: è Costituz. del Regno. See also, Muta capit: 49 del Re Giacomo G. I.

I am, Sir,
Your obedient Servant,
J. BOSTOCK.

Knotshole Bank, near Liverpool,
Aug. 22*d*, 1812.

Dr. Marcet to Dr. Bostock.

" *London, August the* 19*th*, 1812.

" MY DEAR FRIEND,

Controversy on the alkali in the animal fluids.

" I feel much indebted to you for the remarks you have made, and the doubts you have expressed, in some of your last letters to me, respecting the nature of the uncombined alkali in the incinerated salts of serum; they have induced me to reconsider the question, and to add to my former inquiry on that head, a few new results, which, I flatter myself, will remove every shadow of doubt, which may remain on your mind in that respect.

Why no new data brought forward in the last letter.

" In my reply to Dr. Pearson, in March last, I abstained purposely from bringing forward any new data, because the chief object of that letter was to vindicate former statements and inferences; and to show, that there had not been, as was argued by my opponent, any blunder in the mode of reasoning by which I arrived at my conclusions. Indeed, it appeared to me hardly necessary to push the inquiry any farther; and I must own that, from the manner in which Dr. Pearson had thought proper to carry on the controversy, in his two letters on the subject*, I should have felt great reluctance to resume the discussion, had it not been for your interference.

Source of Dr. Bostock's doubts on the subject.

" Your objection, or rather your scepticism, arose from your having found in a mass of salts from serum, (by the successive agency of acetic acid, alcohol, and tartaric acid,) such quantities of potash, as appeared to you to show, that the uncombined alkali was potash, and not soda; and you were farther confirmed in this belief by observing, that the alkaline residue obtained by heating the acetat to redness, was deliquescent. You will see, however, by the following statements, that you were mistaken in your inference; and you will, I make no doubt, admit, that the potash, which you found in the alcoholic solution, must have been in the state of muriat; and that the deliquescent quality of the alkaline residue must have arisen from your acetat having been but imperfectly decomposed, on account of the too low degree of ignition to which you had exposed it, and perhaps also (as you have since yourself observed) in consequence of the presence of muriatic salts. But your experiments appear to show, that the proportion which the muriat of potash in the blood bears to the muriat of soda, is greater than I had at first imagined; and that we had both underrated the power of alcohol to dissolve muriat of potash.

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* See this Journal for February and May last.

Large proportion of muriate of potash in the blood,

The uncombined alkali is soda.

"As to the point at issue, however, namely, the nature of the uncombined alkali, in the incinerated salts of blood, the experiments upon which I think myself warranted to repeat, with increased confidence, my former opinion, that the alkali is soda, and not potash, were conducted in the following manner.

Experiments proving this.

"After evaporating some human serum to siccity, incinerating the residue, dissolving in water the soluble saline substances contained in the incinerated mass, filtering this solution, and evaporating it again, the alkaline mass of salts thus obtained was treated with acetic acid, and afterwards digested with 5 or 6 times its weight of alcohol of the specific gravity of 0·815. The highly deliquescent residue, deposited by the evaporation of the filtered alcoholic solution, was then made red hot in a platina crucible, and kept for a few minutes in a state of igneous fusion. A carbonaceous alkaline mass remained in the crucible, which, after being exposed to the air for 48 hours, in a room without fire, and in damp though warm weather, did not exhibit the least vestige of deliquescence. This mass, the quantity of which amounted to 4 or 5 grains, being dissolved in a little water, was divided into four portions; *a*, *b*, *c*, *d*.

"The portion *a*, being examined by re-agents, exhibited the following properties.

"1. It contained abundance of muriatic acid.

" 2. When suffered to evaporate spontaneously in a glass capsule, it left, at the end of 12 hours, a dry efflorescent crystalline substance, which consisted principally of feathery crystals, amongst which were discerned groups of rectangular plates, and a few minute cubes.

" 3. The presence of potash in this crystalline mass was made obvious, both by the tartaric acid, and by oxymuriat of platina, though not so much so by the latter of these tests.

" The portion *b*, was saturated with sulphuric acid, and submitted to spontaneous evaporation. The result was a rim of confused crystals, surrounding a group of regular efflorescent prisms of glauber, being (at least some of them) terminated by distinct dihedral summits, and having sufficient magnitude to be identified by the naked eye, even at the distance of a few yards; they were made to crystallize over and over again, always with the same result; but in some of these crystallizations, a few crystals of sulphat of potash also appeared, the form of which was not equivocal.

The portion *c*, being treated with nitric acid, yielded by evaporation great numbers of rhomboidal crystals, perfectly distinct to the naked eye, and amongst which no form, at all resembling that of nitre, could be detected.

" The portion *d*, being treated with oxymuriat of platina, the usual crystalline appearance of potash-muriat of platina took place immediately; but by slow spontaneous evaporation, other and more abundant needle-shaped crystals of soda-muriat of platina made their appearance.

" My conclusion therefore, (which I hope will now also be your's) is precisely as before; namely, that the potash which exists in the animal fluids, is in the state of muriat, and that the whole of the uncombined alkali is soda; and as it is a known fact, that muriat of potash is in some degree soluble in alcohol, the circumstance which led you into error is readily explained.

" I have only farther to add, that the fact, which I have endeavoured to establish by a specific inquiry, ought to have been inferred from principle; for it is well known that potash has a stronger attraction for the muriatic acid than soda; and indeed I understand that it is a common process, in some manufactories, to obtain soda by the action of potash-lye on muriat of soda.

" Believe me, ever, &c. &c.

" ALEXANDER MARCET.

Soda from Bullock's blood in large quantity.

" P. S. Since the above was written, I have, in consequence of your suggestion that the blood of Graminivorous animals might perhaps yield potash instead of soda, on account of their living exclusively upon vegetable food, examined bullock's blood, with a view to ascertain this circumstance; and as there was no difficulty in procuring any quantity of that blood, I had some gallons evaporated, from which I procured some ounces of salts, in order to satisfy those who think that nothing certain can be inferred from experiments upon a small scale. However, the results were precisely similar, except that the crystals of sulphat and nitrat of soda, obtained by the processes above detailed, were of much larger dimensions than in any of my former experiments."

XI.

On the Culture and Preparations of Hemp in Dorsetshire, and on the Growth of Sea Cale: by H. B. WAY, Esq.*

DEAR SIR,

Information respecting hemp desirable.

AS you informed me, when you were lately in Dorsetshire, that the Society of Arts, &c. were anxious to obtain information concerning the culture and preparation of hemp in this neighbourhood, I am induced to send you some account thereof.

In what parts of England

I fear my memorandums on the subject will not be worthy the notice of the society, and I should scarcely have ventured to have put pen to paper upon it, if I had not uniformly found that the persons who are concerned in the growth and management of that article are shy of giving information. If what I have sent should induce persons equal to the task, to make the needful inquiries in this county, Somerset, Suffolk, chiefly cultivated.

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* Trans. of the Soc. of Arts, vol. XXXI, p. 63.

Wheat after it without manure.

Sea cale valuabler and not difficult to cultivate.

and Norfolk, (which I believe to be the parts of England where hemp is most cultivated,) and make the culture more generally known that it now seems to be, I shall be much gratified. I hope, if you again visit this neighbourhood, to show you a very fine crop of wheat on the field where you last year saw the persons employed in collecting the male hemp; also another large field of exceeding good wheat, that produced hemp last year, neither of which have had any fresh manure upon them, since the hemp was taken from the fields. I have added some observations on the growth of Sea Cale: this useful vegetable, growing naturally on some of the cliffs near Bridport Harbour, and being one of the most valuable esculent plants that I know, I have found the culture of it in the kitchen garden more easy to manage than has been generally supposed.

I have sent different specimens of the seed, and some of the natural soil, for inspection:

And remain, Dear Sir,
Your friend and obdient Servant,
H. B. WAY.

Bridport Harbour, March 1*st*, 1811.

Account of the Culture and Preparation of Hemp in Dorsetshire.

Preparation of ground for hemp.

Quantity of seed.

Drawing the male hemp.

The female.

Hemp is usually sown about the 15th of May, on the best arable land, on which about twenty cart-load of good rotten dung has been spread, say about a ton to the load. This is well ploughed in, and the ground well ploughed two or three times, and well dragged and harrowed, to get the soil as fine as possible, and about two bushels of seed, or two and a half, sown to the acre. What produces no seed, called by some male or summer hemp, and by others cinner hemp, is drawn about five or six weeks after the plant comes up. It is at that time in blossom. When drawn, it is tied up in bundles, and carried to some meadow land, and there spread to ripen: when ripe and dry, it is bundled and stacked. What stands for seed has no flower that can be discovered; it is the female hemp, and is generally ripe early in September; when it is drawn, bundled up, and stowed up in the field, for the seed to dry and harden, when it is thrashed out in the fields. Most commonly in Dorset the seed is sold on the spot, at from 2s. 6d. to 7s. per bushel; an acre of hemp produces eighteen or twenty bushels. In Somerset they have sometimes thirty bushels of seed to the acre. In the sowing season I have known 21s. per bushel paid for seed. When thrashed the hemp is carried to the meadows, and spread to ripen as the other, and stacked in the same way, to prepare it for sale; it is sent to the houses of the poor in the parishes round which it is raised, to be what is called scaled; that is, each separate stalk of hemp is broken in the hand, and the hemp, which is the outside rind or bark, is stripped off; in which state it is sent to market. The scaling is the employment of old men, women, and children, and of the whole of the labouring family in the evening, as in winter they make but poor wages of it; and one principal inducement for them to do it is, that the woody parts of the hemp make them a fire, but it soon burns out. Complaints are made of a great deal of the hemp being often wasted from improper management, and want of care in the scaling of it. At the Comptons and Bradford, a good deal more hemp would be raised if they could get it scaled, which they find much difficulty in doing; and if it were possible to construct a mill that would swingle it at a moderate expense, on some such plan as the flax swingling mills, and to afford some encouragement to the erecting them, as well as flax swingling mills, it would encourage the growth of both articles materially. An acre of hemp in a good season will produce 14, 16, or 18 weights, of 32lb. to the weight, in Dorsetshire; in Somersetshire they reckon their weight two pounds less, and they sometimes get as much as 35 weights to the acre. The price of the weight of hemp is from 16s. to 20s. per weight. The rotation of crops as follow:

Seed.

The hemp thrashed

and scaled.

Complaints respecting this.

A mill for the purpose desirable.

Produce per acre.

Rotation of crops.

On ground well manured,
Hemp.
Wheat.
Barley or Oats.
Clover with the above.
Wheat.

Barley or Oats.
Ground well manured, Hemp.

But sometimes they dress the ground well for hemp every third year. The quantity of hemp sown in Dorset, is very trifling in comparison to what is sown in Somerset. In the former it is chiefly confined to eight or nine parishes; whereas very large quantities are raised in Somerset, in the parishes of Misterton, Crewkerne, Hinton St. George, Lopen, Seavingtons, Ilminster, Stocklinch, Donyatt, Kingstone, Shipton, Beauchamp, Barington, South Petherton, Martock, Norton, Chiselborough, Stoke-under-Ham, Montacute, Odcombe, the Chinniocks, the Cokers, the Comptons, Bradford, and a great many other parishes. Mr. Emanuel Pester, of Preston, near Yeovil, is in the middle of the hemp and flax county; and he can doubtless obtain and give every information that may be wished on the subject, being so extensively engaged in agricultural pursuits himself, and so competent to give that sort of information wanted. A bounty of 3*d*. per stone on hemp, and 4*d*. per stone on flax, was for many years given by government, but is now discontinued; it was paid by the clerk of the peace for the counties, and as the late Mr. Wallace managed that for the county of Dorset uncommonly well, it is most probable, that a very correct return for the county of Dorset could be obtained from the office of the clerk of the peace for this county, of the quantity raised each year of both articles, during the continuance of the bounty; also from Devon and Somerset similar returns could be got. There are large quantities of hemp raised in Suffolk, the writer thinks, near St. Edmund's-Bury and Stow-market, in that county. He has been told they make linen so fine of hemp, as to be worth 5*s*. and 6*s*. per yard, and used for shirts in preference to Irish, being considered much more durable and better, so much so, as to induce the Irish to imitate the fabric, and stamp the cloth, Suffolk hemp. It is also raised in Norfolk, in the neighbourhood of Lynn and Wisbeach, but it must be watered and prepared in some other way; indeed he is convinced that all the hemp imported from the Baltic is prepared differently from the mode used in Dorset and Somerset, and must have been swingled before it was sent to the different ports it was shipped at for this country. The giving the former bounty on the growth, and

Hemp and flax much cultivated in Somerset.

Bounty formerly given on them.

Very fine linen from Suffolk hemp.

Bounty should

and increasing it on hemp and flax, would encourage the growth; but if given on the number of acres sown, the grower, as his ground would be in high order for a crop of turnips and wheat after, might be careless about his crop of hemp, as the bounty, to be worth notice, must be worth more than the value of the seed in common years and the labour of sowing.

be on the produce, not the measure of the land.

Hemp in this county and the next is never sown in new ground fresh broke up, but flax always by choice, when fresh ground can be got. Mr. John Pitfield is going to break up great part of the West Clift at Bridport Harbour, and sow it with flax this season. The writer, while on the subject of hemp, is led to mention, that when travelling in the year 1792, in the province of Massachusets, near Boston, in North America, he was assured that considerable quantities of hemp were raised in the township of Sunberry, about ten miles from Boston; and that it was always raised on the same ground every year, no other crop being sown in their hemp lands, and that it was manured every year, at the rate of about ten tons of manure to the acre of hemp. Respecting seed, he cannot learn that there is any for sale at Bridport, with the buyers who purchase it up for the growers at the hemp harvest, and he expects that very little can be got from the growers round here. Somersetshire is a more likely place to get it, as he has known some of the hemp farmers to have upwards of a hundred acres of hemp in one season. Round this they generally are only in a small way. A change of hemp-seed is much wanted in Somerset and Dorset. Trials have been made two or three times to get it from Russia, but it is not possible to get new seed from the interior early enough in the fall at the shipping ports, and some old seed which has been shipped has not answered the purpose; if new could have been got, it would as generally have been used for a change, as the new Riga barrel flax seed is by the flax-growers. As the seed sown in Russia was considered a good sample, and its appearance much liked, possibly it might, at a future period, be obtained in the fall from Odessa, or some other port on the Black Sea; as it is understood that a good deal of hemp shipped at Riga and St. Petersburgh grows much nearer to the Black Sea than the Baltic; or possibly the seed of the Italian hemp raised in the neighbourhood of Bologna, or that of America, might be obtained

Flax sown on new ground, hemp not.

Hemp sown constantly on the same land.

Change of seed wanted.

Suggestions for obtaining it.

A previous crop of vetches.

obtained in time to answer. Perhaps tares, called by some vetches, might be cleared from the ground early enough for manuring and sowing the ensuing crop of hemp, and vetches might make it worth the farmer's attention; to this an objection was stated, which I do not just now remember. On talking with the gentleman before-mentioned, and stating the American practice, with what had passed on it with my neighbours, he said, he had long been persuaded, that it was a good practice; and that he had the last season a very good crop of hemp on a piece of ground that had hemp the year before, and that he did not let the hemp stand for seed, but had it all down at the usual time for drawing the summer or male hemp, and the ground immediately sown with turnips, which were fed off with sheep, and the ground then slightly manured, and hemp sown again at the proper season; and that he had then, October 27, 1808, a piece of turnips after his hemp, which were worth 6*l.* per acre. It is to be observed, that the acre here meant is the British acre of one hundred square poles, three hundred and four square yards each. The manure mostly used for hemp is good rotten stable dung, which is much preferred to any other, though lime is frequently used; but manufacturers pretend to assert, (with what foundation I cannot say), that they can distinguish a material difference in the quality of the hemp, where lime has been used instead of dung; as from lime they say hemp is more harsh and brittle, and not of such a soft silky quality as where dung has been used. The writer has endeavoured to throw together every thing that occurs to him on the subject of the culture of hemp, which, from being born and residing great part of his life in a part of the county where it has been extensively cultivated for ages, he has been able to collect; but where it is not very easy to obtain direct information, as both the growers and manufacturers are very shy of giving any, under an idea that it might injure their own interest by assisting to extend the culture to other countries. He believes that his statement may be depended upon; but he is no farmer, and therefore the loose hints thrown together here on the subject may not be so clearly and satisfactorily explained as he could wish; but if they in the smallest degree assist in encouraging the growth of an article so essential to the welfare and

Hemp alternated with turnips.

Manure for hemp.

and prosperity of the kingdom, it will afford him the most heartfelt pleasure.

H. B. WAY.

Account of the Culture of Sea-Cale, or Sea-Kale.

Cultivation of sea-cale.

The mode which I consider the best for the culture of sea-cale is to draw lines in a very dry soil and dry situation, on ground with a southern aspect, about two feet one way by about eighteen inches the other, and where the lines cross, to put in three or four good perfect seeds in a square or triangle, about three inches apart. This may be done any time in November or December in open weather; and it will require no other care afterwards but keeping the ground clear from weeds till the autumn of the following year, when all the plants but one of the finest in each square may be taken up, which if wanted will serve to form other beds set the same distance apart. The ground in the intervals of the plants should be dug in the spring and fall of the year, taking care not to injure the plants. The leaves should be left on the plants till they fall off naturally, which will not in general be sooner than the latter end of November. In the autumn of the second year, the same attention should be paid to the plants, and to remove the dead leaves.

Blanching.

In the third year, about the middle or latter end of November, when the leaves had been cleared away, and the ground dug, each plant should be covered over close with a tub, pan, a heap of small stones, coarse cinders, or coarse bark, raised about ten or twelve inches over the crown of each plant, and from about the latter end of February to the latter end of March, the plants will be very fine and fit for use. I prefer that which has been blanched with our round sea-gravel, about the size of large peas or beans, to any other mode whatever. The plants should be cut but once in a year, as cutting them oftener weakens and lessens the size of the plants. If it is not desired to have the plants large, they may be blanched and cut a year sooner.

Sandy soil best for it.

I have sent a specimen of the sandy soil in which it grows naturally here, as I think the generality of gardeners are too careful, and manure the ground too highly for it. In the month peutics at eight, the practice of physic at half after eight, and the chemistry at a quarter after nine. By George Pearson, M. D. F. R. S. Senior physician to St. George's Hospital; of the College of Physicians, &c.

Clinical lectures are given as usual on the patients of St. George's Hospital every Saturday morning at nine o'clock.

Lectures on Surgery, Physiology, and Pathology.

Lectures on surgery, physiology, and pathology.

Mr. A. Carlisle, F. R. S. professor of anatomy in the royal academy, and surgeon to the Westminster hospital, will begin his course of lectures on the art and practice of surgery, and the sciences connected therewith, on Monday, October the 12th, at half after eight, P. M., at his house in Soho Square.

The introductory discourse is open to all professional students, and the subject to be continued on Mondays, Wednesdays, and Fridays, at the same hours.

The diseases and accidents allotted to the province of surgery will be amply treated of, and illustrated by cases from the lecturer's experience. A compendious view of the animal economy will be adduced to illustrate the several processes of disease, and of recovery.

The operations of surgery, and the anatomy of the affected parts, are to be demonstrated.

Surrey Institution.

Lectures at the Surrey Institution.

We understand the following arrangements have been made for lectures at the Surrey Institution, in the ensuing season; Mr. COLERIDGE on the Belles Lettres, to commence on Tuesday, the 3d of November, and to be continued on each succeeding Tuesday; Mr. MASON GOOD on the philosophy of physics, to commence on Friday, the 20th of November, and to be continued on each succeeding Friday; and Dr. CROTCH on music, to commence early in 1813.

London Hospital.

Practice of physic.

Dr. Buxton's autumnal course of lectures on the practice of medicine will be commenced on Thursday morning, the 1st of October, at 11 o'clock.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

NOVEMBER, 1812.

ARTICLE I.

A Continuation of Experiments on the soniferous Vibrations of the Gasses, &c. by Messrs. KERBY *and* MERRICK.

To William Nicholson, Esq.

SIR,

THE following experiments, on the musical sounds of the gasses, were performed with new apparatus, similar to that which I described in a preceding communication to the Philosophical Journal, vol. XXVII, p. 269. To those who may be disposed to construct apparatus for the same purpose, a statement of its dimensions will not be unacceptable. Such, however, as would possess it without the trouble of fitting it up themselves, may procure it of Mr. Bancks, 441, Strand.

Bellows. The bellows are made of three small pieces of mahogany, each six inches long, three wide, and three tenths of an inch in thickness. They are connected by folds of thin leather, glued round their edges. The requisite pressure on the bellows is given by a spring of brass wire. A kind of fusee was added, to equalize the blast; something like a contrivance for the same purpose in Mr. Liston's perfect organ :—on trial it was found in this case, to be of no advantage.

Pipe.
Its pitch.

The organ-pipe employed is of the kind called the stopped diapason: its width is 0·57 of an inch, depth 0·71, and its length, without the plug by which it is tuned, 5·15;—the thickness of its sides 0·15, and the width of the *mouth* 0·16. When tuned a minor tone above one of Hawkins's new C tuning-forks, the length of the vibrating column of air was found to be 4·1 inches; the barometer being at 29·60, and F. thermometer at 65°.

Air pump and guage.

A long barometer-gauge was added to the air-pump, having a scale movable by an endless screw for adjusting the zero to the surface of the mercury in the basin below it. In noting the experiments, the height of the mercury in the gauge was subtracted from that of a barometer suspended in the same room.

Thermometer.
Receiver.
Vapour.
Exhaustion.
Sulph. acid used to absorb vapour.
Ice produced in 2′.

Having by accident broken the small thermometer attached to the bellows-frame, we registered the temperature from another thermometer placed outside of the receiver. The capacity of the glass receiver is 275 cubic inches, and that of the effective part of the pump barrel 26·7. Nothing but pomatum was used between the brass plates and the receiver, on account of wetted or oiled leathers being known to afford a great deal of vapour. Notwithstanding this, the gauge was depressed by vapour; for, on exhausting the receiver, the gauge indicated two tenths of an inch less than the barometer, but when again exhausted, after placing a cup with sulphuric acid in the receiver, the difference of the two was only 0·06, and in one case we could perceive no difference. For this reason, in all the experiments, except those on water, alcohol, ether, and oil of turpentine, a glass containing five or six ounces of sulphuric acid was placed by the bellows in the receiver. With this pump, and using the acid instead of muriate of lime, water has been frozen by Mr. Leslie's process in two minutes, while the thermometer in the room was at 67°.

Plate.

Plate V fig. 1 (opposite page 240 of the present volume) represents the apparatus made use of for transferring liquids into the exhausted receiver. It consists of a small glass tube, graduated, which screws on the cock above the transfer-plate: the top of this tube is closed by a piece of ground glass smeared with pomatum. To the other end of the cock, under the plate, a small semispherical brass dish is screwed, to catch any liquid that falls through the cock, and prevent its injuring the bellows.

Monochord.

Its wire.

Should be of steel.

Beats.

Every time, before the receiver was exhausted, the wooden pipe was carefully tuned to 0·2250 of a monochord or sonometer, divided decimally, the monochord being tuned accurately to a C fork. The box of this monochord is made of straight-grained deal, and is 36 inches long, three inches wide, and 2·5 deep. Over two immovable bridges, placed 30 inches asunder, a steel wire, 0·017 of an inch in diameter, is strained by two endless screws, placed at the extremities of the box, which act like the screws of a modern English guitar. A long wire is preferable to a short one, because a small alteration of the tension or temperature will cause a less perceptible difference in the pitch of the sound it produces. Lord Stanhope used steel wire on his curious monochord, finding that it did not keep continually lengthening, as brass or iron wires do when the tension is considerable. A curious experiment is related of the Stanhope monochord, which I have never yet seen explained. Two equal wires were put on it, and brought in unison with G, an octave below the treble cliff. One of the wires was then shortened as little as the eighteen-thousandth part of an inch, and this was said to produce invariably an audible *beating*, which could be very sensibly *felt* with the finger as well as heard! What was the cause of this beating? The length of the G wire was 20 inches, which could be divided by that instrument into 360000 equal parts, consequently the length of the altered wire was 359999 of those parts. Now, the vibrations of strings, which differ in length only, being in the inverse ratio of their lengths; if we assume 180 as the number of vibrations in 1″ of that G at concert pitch, the shorter string will make only 180·0005 vibrations in 1″, and consequently not a single beat can be produced by such an imperfect unison in half an hour* ! The beating that was produced, therefore, remains unaccounted for.

Concert pitch.

* Sauveur, Chladni, and Dr. T. Young, consider every *Ut* of C as a power of 2, taking the fundamental C for unity. At this pitch, middle C makes 256 "acoustic vibrations" in 1″: Sauveur's experiments, in 1700, give 244. Euler and Marpurg attribute to the same C 236 and 250 in 1″; Cavallo gives 256·8; Smith 247; Sarti 262; Robison 240; Hawkins 238·6; and Farey 241·5.

Tension of the wire.

Doctor Crotch remarks*, that a monochord-wire should be stretched equally at both ends, or else it will be inaccurate.

Chladni and Jacquin's experiments.

Their apparatus.

The only experiments on the sounds of the gasses, with which I am acquainted, and which have preceded ours, are those made by professors Chladni and Jacquin at Vienna several years ago. Many objections might be made to their apparatus. It consisted of an open organ-pipe of pewter, fixed within the neck of a glass receiver, furnished with a stop-cock above the pipe, and a bladder on the outside. When the apparatus was sufficiently filled with gas, the blast was excited by pressing the bladder: this was done over water. The temperature during their experiments was from 54° to 59° of Fahrenheit. The length of the vibrating column of air in their pipe was about 15 *centimetres*, or 5·9 inches; hence it would produce a sound of three octaves higher than *Ut* 3, or the tenor-cliff C. Their results will be mentioned farther on.

Length of the pipe;

pitch 11331/5·9.

Priestley and Perolle's exp.

The experiments of Priestley and Perolle, with a bell rung by wheel-work, had for object only to determine the *intensity* with which sound is transmitted by different kinds of gas, and are therefore dissimilar from those which I shall now describe.

Exp. 1.

Nitrous oxide.

1. The receiver being exhausted till the gauge stood only 0·44 of an inch lower than the barometer, *nitrous oxide*, produced by decomposing nitrate of ammonia, was transferred from the gas-jar into the receiver of the pump in four successive quantities. After each transfer, the scale of the gauge was adjusted, and the movable bridge of the monochord slid till the wire and pipe were in unison. While this was doing, the gauge ascended a small quantity, as we had anticipated, from the absorption of vapour, by the sulphuric acid. To save room, as the mode of operating was uniform, I shall dispose each gas in a separate table of five columns, the first, from the left, showing the number of successive quantities, and the name of the gas; the second, the temperature; the third, the quantity of rise; the fourth, the pressure after that rise was observed; and the fifth, the monochord lengths corresponding with the pitch of the organ-pipe. No settled portion of time was allowed for the gauge to ascend. The sound of this was louder and deeper than that of any other gas, and in quality of tone

Explanation of the tables.

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* Elements of Thorough Bass, &c. 4to. 1812.

(*timbre*) resembled the sound produced by a bad-toned bagpipe. The pitch was a little more than a major third below that of atmospheric air: our preceding experiments with an *open* pipe and less accurate apparatus gave 3d.—comma.

1. Nitrous oxide - -	67	·07	8·17	·27350
2. - - - - - - -	67	·11	15·52	·28000
3. - - - - - - -	67	·20	23·02	·28550
4. - - - - - - -	67	·25	29·77	·28675

We thought the acid in the receiver was become a little more opaque, but no froth appeared on its surface, as it had in some of the experiments. On opening the stop-cock in the transfer-plate, and working the pump, the sound of the pipe became more acute, like the sound of a violin-string, which is slowly shortened by sliding the finger.

Expt. 2. Carbonic acid.

2. *Carbonic acid*, disengaged from chalk by dilute sulphuric acid, and collected over water. The tone of this gas was weak and reedy[*].

1. Carbonic acid -	61·5	·30	10·23	·2740
2. - - - - - - -	——	·12	14·25	·2772
3. - - - - - - -	——	·34	25·96	·2850
Atmos. air - - -	——	—	28·80	·2850

Exp. 3. Carbonic acid and hidrogen.

3. This gas remaining in the receiver, the pump was worked till the pressure on the gauge was 14·34, when a quantity of hidrogen gas was added. The sound became more acute with a slide, and clearer and smoother than the sound of carbonic acid alone.

Carbonic acid - - -	61·5		14·34	·2760
Hidrogen added - - -			28·80	·2115
Do. remained - - -		.65	28·15	·2125

The pitch of these gasses was not altered by working the pump till the pressure was 14·46; but on filling up the receiver with atmospheric air, the sound was depressed to ·2215, and the gauge rose 0·30 in a very short time.

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[*] Chladni found the pitch of this gas to be almost a major 3d below that of atmospheric air, a result which accords with this experiment.

4. *Chlo-*

Exp. 4. Oximuriatic gas.

4. *Chlorine* gas, obtained from oxide of manganese and muriatic acid, and collected over water. In this experiment the gauge was not used. The receiver was filled by three successive and nearly equal quantities of the gas. The pitch was not quite a minor 3d lower than that of air.

1. Chlorine gas - -	67	——	——	·25400
2. - - - - - - - -	—	——	——	·27675
3. - - - - - - - -	—	——	——	·27900

Exp. 5. Olefiant gas.

5. *Olefiant gas*, or supercarburetted hidrogen, produced by boiling, in a glass retort, alcohol and sulphuric acid.

1. Olefiant gas - - -	60	·09	7·39	·23400
2. - - - - - - - - -	59	·13	15·65	·23825
3. - - - - - - - - -	59	·16	22·62	·24100
4. - - - - - - - - -	59	—	29·18	·24120

Its pitch.

Exp. 6. Olefiant and oximuriatic.

6. The pitch, taking the mean length, is almost a major semitone below that of atmospheric air. Having noted this experiment, the pump was worked till the pressure was only 15·68, and three measures of *chlorine* gas were added, in succession, to the *olefiant gas*, which remained in the receiver.

1. Chlorine added - -	55	2·70	19·48	·25650
2. { - - - - - - -	54	- - -	26·14	·26000
{ remained - - -	54	4·39	21·76	·25550
3. { - - - - - -	53	- - -	27·75	·27400
{ - - - - - -	53	6·41	21·34	·26925

Pitch.

During this experiment, the barometer ascended from 29·18 to 29·75. The tone was very peculiar, and cannot be easily described. After each addition of gas, the pressure began to diminish, and the pitch to ascend. The mean length gives the pitch nearly a superfluous second lower than that of atmospheric air. The brass plate of the pump was found entirely covered with a purplish gray oil, which was extremely difficult to remove, and which was not quite removed for a long time after.

Exp. 7. Oxigen gas.

7. *Oxigen gas*, obtained from oxide of manganese by heating it in an iron retort.

1. Oxigen gas - - -	69	.03	7·95	·23375
2. - - - - - - - - -	69	·10	15·81	·23600
3. - - - - - - - - -	69	- -	22·61	·23900

Here the lever of the bellows breaking, we had to begin this experiment anew. The gas was pumped into a bladder and used again.

1. Oxigen gas - - -	69·5	·05	8·26	·23250
2. - - - - - - -		·25	15·61	·23600
3. - - - - - - -		·40	22·61	·23800
4. - - - - - - -		- -	29·98	·23875

Its pitch.

A mean of seven gives the pitch a little more than a minor semitone graver than that of air—Chladni found it to be a semitone or nearly a tone; and our former experiments, with the little open pipe, make it not quite half a comma.

Exp. 8. Nitrogen gas.

8. *Nitrogen gas,* obtained from small pieces of lean muscular flesh (beef) and weak nitric acid, gently heated in a glass retort. The gas stood over water for twelve hours before it was used.

1. Nitrogen gas - - -	68	·08	7·72	·2185
2. - - - - - - -	68	·14	15·20	·2250
3. - - - - - - -	68·5	05	22·95	·2275
4. - - - - - - -	69	- -	30·00	·2290

Its pitch.

This gas produced a very weak, dead sound; the same in pitch as common air. Chladni found it almost a semitone graver.

Exp. 9. Nitrogen and oxigen.

9. After working the pump till the pressure was only 22·5, *oxigen gas* was added to the nitrogen in the receiver, till the pressure was again 30·0, when the sound of the mixed gasses was 0·2335, or almost three commas below the sound of common air. Chladni found that a mixture of these two gasses gave a sound in unison with that of atmospheric air, being more acute than either gas alone. "But before the mixture of these fluids had become homogeneal by repeated pressions of the bladder, the sound was not appreciable, because the vibrations could not be isochronous." (Chladni, § 67.)

Exp. 10. Nitrogen.

10. *Azotic gas,* procured by setting fire to a piece of phosphorus in atmospheric air confined over water. It was inflamed with a burning lens. Before the gas was used it was left in sunshine, over water, for several hours.

1. Azotic gas	60	·10	7·30	·2210
2.	—	·10	14·50	·2210
3.	—	·10	21·55	·2215
4.	—	·15	27·75	·2230
Added common air	—	- -	29·40	·2225

(A little air added.)

Pitch.

A mean of the four gives the sound a little more than a comma more acute than the sound of common air.

Exp. 11. Sulphuretted hidrogen.

11. *Sulphuretted hidrogen*, obtained from powdered sulphuret of iron, a little water, and weak muriatic acid, gently heated in a glass retort, and collected over water. A large quantity of the gas was absorbed by the water in the pneumatic trough.

1. Sulphuretted hidrogen	62·5	·39	6·88	·0000
2.	61·5	·68	13·74	·2035
3.	61	·30	20·82	·2070
4.	58·5	·50	27·12	·2075

The least sonorous of the gasses.

This is the least sonorous of all the gasses that we have tried. The sound was hardly appreciable after the second tranfer of gas; and even after the third it was impossible to maintain a continual sound by the most rapid action of the bellows. The pitch of this gas, from a mean of the three lengths, is not quite a minor tone higher than that of common air, but is more than a major semitone. At the end of this experiment, the sulphuric acid placed in the receiver had a froth on its surface about a quarter of an inch high, and of various metallic colours. And we observed, that a part of the oily matter produced in the sixth experiment, and which so obstinately adhered to the plate, could be much more easily wiped off.

Its pitch.

Exp 12. Hidrogen.

12. *Hidrogen gas*, obtained from water, bits of zinc, and sulphuric acid.

1. Hydrogen gas	61·5	·10	7·96	·111
2.	——	·08	15·36	·111
3.	——	·14	22·72	·111
4.	——	- -	28·86	·111

Its pitch.

The sound was weak, and more than an octave and comma above that of air. On working the pump till the pressure of the gas was 14·28, and adding breath till the pressure was again 28·86, the sound of the mixture agreed with ·1815 of the monochord; and the gauge soon rose again 0·2.

Breath added.

14. *Light*

Exp. 13. Light carburetted hidrogen.

13. *Light carburetted hidrogen*, produced by distillation from chips of deal.

1. Lt. carb. hidrogen - -	59	- -	- - -	·1075
2. - - - - - - -	—	11	14·92	·1075
3. - - - - - - a	—	9	21·55	·1075
4. - - - - - - -	60	- -	28·45	·1100
5. - - - - - - -	—	- -	29·13	·1120

The pump being worked till the pressure of the gas was again 14·92, the sound was ·1060. The pitch, from a mean of the six monochord-lengths of wire, is more than an octave and two commas above that of air.

Its pitch.

Exp. 14. Sulph. ether.

14. *Ether*. The graduated glass tube was screwed upon the stop-cock of the transfer plate, and filled with ether. The pressure on the gauge being 0·62, with no acid in the receiver, a small part of a cubic inch of ether, as shown in the left-hand column of the following table, was admitted into the receiver through the stop-cock.

·05	1. Ether - - - - -	60	0	1·48	·09650
·05	2. - - - - - - -	—	- -	2·05	·10 ·20
·05	3. - - - - - - -	—	- -	3·09	·10387
·05	4. - - - - - - -	—	- -	4·30	·11000
·05	5. - - - - - - -	—	- -	5·30	·11290
·05	6. - - - - - - -	—	- -	6·10	·11450
- -	7. - - - - - - -	—	- -	8·77	uncert.
·00	Air added - - - -	—	- -	29·70	·22150

The receiver heated.

After the seventh transfer of ether, we applied a cloth dipped in warm water round the receiver. The gauge fell 0·38; and we imagined we could hear two sounds, about a fifth different in pitch; the one a wheezing tone, the other much clearer. The hemispherical dish was about half full of liquid ether within the receiver. By a mean of six lengths, the pitch is almost an octave and major semitone more acute than the sound of atmospheric air. The air and ether vapour give a sound which is only about a comma-deficient minor semitone more acute.

Exp. 15. Alcohol.

15. *Alcohol*, introduced into the receiver in the same manner as the ether, sunk the gauge only 0·55, and produced no sound. When air was added to fill the receiver, the sound was 0·2260, and very indistinct. Thermometer in the room 60°, barometer 29·7.

Hot

Other expts. Hot water, liquid ammonia, and oil of turpentine, were successively treated like the ether, and found to produce no sound, and but very little depression of the mercury in the gauge.

Air and hidrogen gas are the only elastic fluids that have not varied in pitch with a considerable variation of pressure.

At present, I shall not enlarge on these experiments; but subjoin a table, showing the *relative* lengths and vibrations corresponding with the sounds of the gasses when the sound of air is taken as unity. "Des faits, et point de verbiage, voilà la grande règle en physique comme en histoire." (Dalembert.) In the right-hand column of the table I have placed the logarithms of the intervals with air; for the value of any interval is the logarithm of its constituent ratio*.

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* See Dr. Smith's Harmonics, sect. I; and a Table of Intervals, by Mr. Farey, in the Edinb. Encyclopædia, vol. II, 1810.

Postcript. The pitch or value of a sound depends on the frequency of its vibrations. It has been asked—"Why should nôt the measure of an interval be the *difference* of the values of its terminating sounds? and consequently why should not intervals be compared by the differences of the values of their sounds?" In answer it has been said, that "the measure of an interval estimated in that manner would vary according to the unity of time chosen for representing the value of the sounds. For, let *a* and *b* be the numbers of vibrations of two sonorous bodies in one second; *ma* and *mb* will be the numbers of vibrations of these bodies in a time *m* times greater. The interval would then be measured in the first case by *b* — *a*, and in the second case by *mb* — *ma*, a quantity necessarily different." The following theorems respecting intervals, translated from A. Suremain Missery (1793) may be useful to some of your readers, who are not familiar with the subject.

"Considering intervals in one direction only:—

Theorems. "I. The product of the constituent ratios of two or more different intervals is the constituent ratio of the interval which would be equal to their sum.

"II. The quotient of the constituent ratios of the two intervals is the ratio constituting the interval which would be their difference.

"III. Every natural power of the constituent ratio of an interval is the ratio constituting the interval which would be a multiple of the first marked by the degree of the power.

"IV. Every natural root of the constituent ratio of an interval is the constituent ratio of an interval that would be an aliquot part of the first marked by the degree of the root.

"V. Any fractional power whatever of the ratio constituting an interval is the constituent ratio of the interval which would be a portion of the first marked by the exponent of the fractional power." 75. For authors on this subject, see Forkel's *Allgemeine Litteratur der Musik*, *kap.* I. of the second part; Leipsic, 1792.

Aeriform fluids.	Mean lengths of wire.	Relative lengths.	Relative vibrations.	Logarithms of intervals.
Nitrous oxide gas -	·2814375	1·250833	0·799467	·0971995
Carbonic acid - -	·2787333	1·238814	0·807223	·0930063
Chlorine - - -	·2699167	1·199610	0·833590	·0790473
Do. and olefiant - -	·2630500	1·169111	0·855351	·0678558
Olefiant - - -	·2386125	1·058333	0·944882	·0246225
Oxigen - - - -	·2362857	1·050159	0·952237	·0212549
Do. and nitrogen -	·2335000	1·037778	0·963597	·0161044
Carb. a. & hidrogen	·2333333	1·037037	0·964286	·0157942
Common air - - -	·2250000	1·000000	1·000000	·0000000
Nitrogen - - -	.2250000	1·000000	1·000000	·0000000
Azotic - - - -	·2216250	0·985000	1·015228	·0065637
Ether vap. and air -	·2187500	0·972222	1·028571	·0122344
Sulph. hidrogen -	·2060000	0·915556	1·092233	·0383153
Hidrogen - - -	·1110000	0·493333	2·027027	·3068595
Carburetted Do. - -	·1084167	0·481852	2·075326	·3170863
Ether vapour - -	·1063283	0·472570	2·116088	·3255337
Do. highest - -	·0965000	0·428889	2·331606	·3676552

I remain,
Sir,
Your humble Servant,
ARNOLD MERRICK.

ueen's Road, Cirencester,
22d Sept. 1812.

II.

n the secret and open Nectaries of various Flowers. In a Letter from Mrs. AGNES IBBETSON.

To Mr. Nicholson.

SIR,

On the nectary of flowers.

IN the exact dissection I have given of a flower in your Journal for July last, the explanations of the calyx, corolla, d stamen, were alternately given, their peculiar vessels deribed, and the separate cylinders, which convey those vessels the stalk, accurately and exactly marked. There remains, erefore, of the flower but two parts to develope, the *nectary* and e *pistil*: the most important, indeed; and which have never, think, been rightly explained, or properly delineated, particularly the former. I shall therefore dedicate the greater part of this letter to this subject: first, to the display of the various functions of the nectary; secondly, the importance of this part of the flower to botany in general; thirdly, the description of the many different sorts of nectaries both concealed and open; and fourthly, the curious mechanism displayed in their various formation.

Honey supposed to be intended to invite insects, so that they might convey pollen from one flower to another.

It has been conceived, and frequently asserted, by our first physiologists, that the only purpose, or known use, of the honey found in plants, was to tempt the insect tribe to visit the flowers, that, while inserting their heads into the interior, they might, by rubbing against the stamen, take up some of the powder of the pollen, and convey it to the pistil in other flowers; and thus impregnate seeds, which, without their assistance, might not be able to procure the powder necessary to their completion. That nature has bestowed on the insect tribe the curious knowledge necessary to seek the honey in a flower*, and make their search thus serviceable not only to themselves but to botany, I have no doubt; but I am equally convinced, that few, very few, of the indigenous plants of any country, (even of the diœcian class,) require such assistance; and that if flowers were never removed from their native soil, they are all sufficient to perform every part which nature has assigned them, in fructifying their own seeds. It is very little known, (because it has never before been a matter of serious investigation,) how much motion indigenous plants possess. That they are scarcely ever still, is an absolute truth; and that on a warm day we need not seek in the plants of other countries that curious and regular motion to be found in each field in our own. That the pistil and stamen regularly bend to each other, so as to enable

This assistance seldom necessary.

Mobility of our native plants.

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* Any person, who has seen an insect seek the honey in an antirrhinum, will be convinced, that this knowledge is necessary; and that without it the insect would fly to any other part of the flower, but the one at which it opens: whereas it settles at once on the round top, fixes one foot on the opposite petal, which it pushes open, inserts the head and shoulders within the flower, lengthens the proboscis, and draws up the honey. All this is the work of a few seconds of time, and it is done in so perfect a manner, so immediate and so direct, that nothing but a thorough knowledge of the flower could enable the insect to act thus decidedly.

nale to acquire pollen sufficient to fructify its seeds, there
no doubt with those who thoroughly watch flowers; also
e mechanism for the purpose exists in them, and is always
capable of performing its office in indigenous plants. If,
ne, the pistil and stamen can almost always suffice, with-
l, to impregnate the seeds, is it likely, that so large and
gly important a part as the nectary should be placed in
flower, when not likely to be necessary but to a very few?
e nectary has, in reality, a much more important task to
m, and its history is beautiful and perfect in all its parts.
is in every flower a concealed as well as an open nectary;
cious juice is formed within the vessel of the line of life,
increases in sweetness as the plant advances towards
ing. It is this juice which appears at the head of the
l in one or many glittering drops, which dissolve the pol-
nveying the joint mixture to the seeds, which it impreg-

Nectary not formed for one purpose only.

Without this liquid, therefore, the seeds would not be
ated. Hence the cause of nature's forming *two nectaries*
h flower, which are the sacred deposits of this juice; the
st insects can attain, the other closed to them, and so
guarded when not *wholly closed,* that death generally fol-
he attempt to seize it: for though the insect tribe are
t to find the open one, yet if they could take all the honey
eposited, the seeds could not be impregnated. Thus
e, ever indulgent, bestows as long as the *general good* will
; but gluttony brings its own punishment.

Why two nectaries are necessary.

Farther use of the nectaries.

r are the purposes already mentioned the only ones for
the nectary was designed by nature, and bestowed on
s. The various alteration of the juices; the first feeding
embryo of the seeds and buds; the innumerable combi-
s formed in the interior of plants, of which the nectarious
s the basis; and the decomposition of water, which the
microscope so admirably shows to be constantly going on
nts; must owe much to the juices of the nectary. I have
ed for some time past, that the line of life (the source of
ctary) bleeds whenever it arrives at that part of the stem
the leaves shoot, and the buds come forth; and have
ally seen it ooze out when greatly magnified: but I was
curious to know whether this bleeding was the *cause* of
hange of colour, which so frequently takes place in many

Bleeding of the line of life.

different plants at that time, when even the stalk, as well as the axilla of the leaves, is reddened by its tint. I took out, therefore, the whole cylinder belonging to the line of life, and procured a drop or two of this luscious liquid. I then divided the wood from the same plant, and by pressure obtained a little sap: and the mixture of the two immediately changed the colour of the liquids, and converted the light green of the sap to a dark red colour. That the sap running in the *perfect wood* is almost always a pretty powerful alkali, I have long been convinced (differing from that found in the alburnum:) and that the buds, on the contrary, are more or less acid, may be easily shown by cutting and then immersing them in a weak alkaline liquor. The direct change of colour produced in the wood by passing a feeble electric stroke through a plant also proves it, since it reddens the wood vessels, but has no such effect on the buds. We may therefore, I think, conclude, that the reddening of the stalk is caused by the bleeding of the line of life, and the mixture of the *acid* and *alkali* in the bosom of the leaf: and we may also look to the nectary for three of the most powerful effects observable in the life of a plant.

Effect of the acid and alkali in colouring plants.

I now turn to the third division of my subject, and shall show the various species of nectary found in plants, both concealed and open. I need not here represent the manner in which I dissect flowers, as my former letter has shown it, and proved, I hope, how impossible it is, that after cutting them in three *different directions*, I should be mistaken in the formation of their various parts; since in such a *display* they would mutually be the means of detecting each other, if not critically exact in their delineations. All flowers, with respect to the situation of their seed vessels, may be divided into three distinct kinds: those which have their seed-vessels above the flower; those in which the same part is found much below, and those in which the germe occupies nearly the centre. In the first and second the secret nectary is generally found in a deep cavity either above or below the seed-vessel, according to its situation in the flower; but in the last it is so various, that there is no giving rules to find it. In a monopetalous corolla the secret nectary generally forms a little box under the seed-vessel, which has sometimes the stamen opening into it, and is therefore discovered by drawing out the filaments, and finding their ends

The situation of the secret nectary in monopetalous plants,

steeped

steeped in this luscious juice, when there is not any perceptible in the *cap* of the flower. Another sort of secret nectary is found in most triandrian and hexandrian plants; which have their seed-vessels very low in the stem, and the concealed nectary reaching from the bottom of the corolla to the germe. In some flowers it is above two inches deep; nor is it useless in this situation; it is not only ready to rise to the stigma to impregnate the plant, but it has various vessels passing through the exterior of the seed-vessel (where it joins the nectary) to nourish the embryo till the flower decays, and the rest of the luscious juice evaporates. In the tetradynamian flowers, as well as the geraniums and some others, the *apparent well* is on one side of the stem, reaching from the corolla to a certain mark, or to a stipula; and always distinguished by one of the leaves of the calyx turning *up*, while all the rest turn *down*. The open nectaries of these flowers are as various as the plants. In most of the geraniums it is a *trough* between the corolla and stamen, rising round the pistil, and oozing up from the well below. In the triandrian plants the second nectaries are generally either cavities at the bottom of the stamen, or vessels managed within the corolla. The admirable double nectary of the iris develops much of the intentions of Nature in its formation, since its secret repository is not quite closed, but is sure to catch the insect that attempts to seize its contents. The open nectary is in some species a beautiful fringe with a wide vessel down the reflected petal, and in others it consists of three honey-bearing excrescences flowing from the same source: but the secreted juice is found in a deep cavity in the stem, having a trough at the top, and within the flower, to hold the precious liquid. The two points of this trough secure the insect as it crawls down between the petals, when, not contented with the feast the open one bestows, it stretches out its proboscis to get at the honey within the trough. How often have I stood contemplating this picture, and seen the insect try to insinuate itself lower and lower, though apparently well informed of the danger it was encountering! I found the other day, in a yellow iris on the border of the river, a bee, which, having ventured too far, was caught by the projecting points of the trough, which, like the stamen of the berberris and apocynum androsæmifolium, had got it fastened between them: and it would there have died, had

triandrian and hexandrian plants,

and in tetradynamian plants.

The double nectary of the iris.

The nectary seizing an insect.

had I not released it; for the warmth or moisture of its body so contracted the spiral wire in the petals, as to press them tight against the insect. Nor could it ever have regained its liberty. It was really astonishing to see the flower able to resist the struggles of so large a creature, but the very violence of its exertions seemed to increase its danger, by pressing from below such a quantity of liquid, that it was almost drowning in honey. The best way of getting at the secret nectary of the iris is not by the flower, but by cutting the stem where it is marked for the beginning of the seed-vessel, as that points out also the termination of the nectary.

Nectaries in the diadelphian class.

The two nectaries in all the diadelphian class are most admirably managed. To look at the exterior of the flower it would appear almost impossible to find room sufficient for one, and yet all those flowers possess a very perfect double nectarium, with all the various vessels its different offices require: a deep trough within the cylinder of the males is filled even to half the height of the filaments with this precious juice; and that the bees also may have their share, there is a nectarious ball on each side, on which the banner is fastened, thus serving a double purpose. As the insects draw the honey forth, it is constantly replenished from the trough below; and the exquisite beauty of the contrivance is completed by its not only being ready for supplying the stigma, should the weather be unfavourable to the ripening of the pollen (as in this case it loses much of the nectarious juices by evaporation); but it is also all that time feeding the embryo in the seeds; which have innumerable vessels running through the pod, and most plainly to be perceived in the solar microscope, to imbibe this juice for the nourishment of the young plant; but no sooner does the cylinder of the stamen decay and fall off, and the pods increase, than the skin thickens, the vessels disappear, and the embryo no longer receives nourishment except from the stalk, and its own nourishing vessels.

Didynamian plants, and those with naked seeds.

In the didynamian plants, and in all those flowers which have uncovered seeds, the nectarious juice is secreted in a box in the large part which lies directly under the seeds; while the open nectary is either found at each corner, or opposite to the seeds. In giving the exact picture of the flower of the peach, in the Journal of July last, dissected in three different ways, I left a square at the bottom of the seed-vessel not allotted to any use; it is this which forms the nectarious box, while the interior lining, or inward cylinder of the stamen, forms the open nectary of all the icosandrian class. Wherever this yellow matter is found, (which appears something like softened bees' wax) it always indicates the nectary, as does also a very brilliant white substance found at the top of the seed-vessel in all pentandria digynia plants, which is also a never-failing sign of its presence. If the dissector finds it difficult to discover the secreted juice, there is a little insect, which may be seen with a small magnifier, that always leads to it. It is of various shades of brown, raises its tail, and the half of its body above, smoothing down its very short wings. It lives wholly on the nectary of plants, and is found there alone. When a drawing, or a sort of map is taken at the bottom of each flower, showing the manner in which the vessels branch off to form their appropriate ingredient, it is exactly seen, that not the breadth of a hair is left unemployed and unallotted, and that the most marked adjustment takes place in this respect: and this is done without in the least disfiguring the parts; all appear as perfectly to coincide as if this alone was the object in view, and yet for what great ends, what noble designs! I often throw down my pen and pencil in despair, ashamed of the folly of attempting to give an idea of works almost too great for man even to conceive. It is certainly true, that we are more struck with the power of the Almighty, when we contemplate his small works, than in the view of his larger chefs d'œuvre: when each diminutive object is so highly finished, it would appear the peculiar care of providence; and yet on farther examination of these minor objects, we find all equally perfect and beautiful.

A yellow matter, an indication of the nectary.

Insect living in the nectary of plants.

Beautiful adjustment of the flower.

To give the double nectary in all plants would be an endless business, equally tiresome to the reader and writer; but I think I shall have given (with the following examples) sufficient to prove the truth of all I have advanced—"That there is a concealed as well an open nectary in all flowers;" and that the situation of both is generally regulated by that of the seed-vessel, which appears to connect it indifferently with calyx or corolla, stamen or pistil: but, however this may seem a matter of no moment, I doubt not it is of the *utmost consequence* to the parts, and regulated with the greatest judgment; and that, when I am still better acquainted with the subject, I shall discover the neces-

Nectary connected with the calyx, &c.

necessity of this variation in the situation of the nectaries: for there is not any thing more striking in dissecting plants, than the simplicity and ease with which the cause is discovered in the effect. As a specimen of the nectary in the calyx, I shall give the stock (pl. iv, fig. 1.) Here the two opposite leaves serve as the hidden nectary, and the excrescence alternating the stamen forms the open one. For an example of the nectary in the corolla, I shall give the fritillaria: in this a sort of basin, fig. 2, corresponds with the secret nectary under the seed-vessel. In the stamen it is either at the bottom of the filament, fig. 3, AA, or joined in a more conspicuous manner, as in the corn flower, fig. 4, BB, which is distinguished by many curious circumstances peculiar to itself, which I shall enlarge upon another time, when I give the dissection of the stamen. In all pentandria digynia plants, we have examples of the nectary joined to the pistil and seed-vessel, as at fig. 5: CC being the open, and DD the closed nectary. Here we also see a proof of that brilliant white matter, which always announces the luscious juice. I shall present also one example of a plant having its seed-vessel above, in the passion-flower, fig. 6. Here the nectary is sure to be placed in the following manner: the open one at EE, the closed one at F, perfectly secreted from all danger, yet corresponding with the other nectary, and free to communicate its juices for the benefit of the seeds, and repairing any excess of evaporation lost on the stigma. The secret nectary in the iris I have already shown to be a species of well, see fig. 7, from H to H, while the corners of the trough in the flower are seen at II. The secret nectary in the geranium is shown, fig. 8, from K to K; and the open one, a trough round the pistil at LL. The two nectaries in the silene, cucubalis, lychnis, &c., are displayed at fig. 9: MM being the secret one, NN the open nectary. Those in all flowers, which have uncovered seeds, are seen at fig. 10; PP the open one, OO the secreted box. I have avoided giving those most commonly known, as I preferred the nectaries which have not yet been noticed by botanists, to show more certainly, that two are found indifferently in *all plants*.

Nectary in the corn flower.

Conclusion of the subject.

I shall now conclude my letter with a few words on the mechanical properties of the nectary. There is, I believe, no nectary, that has not the power of giving out its juices, or retaining them,

them. In the delphinium, fig. 11, I have seen it when almost full and turning down, still retain its liquid; nor could I at first conceive how this was managed, till, on dissecting it, I found that there was an inner lining, which contracted at R, when the round part was much distended; and thus prevented the expenditure of its juices. At fig. 5, the nectary has not only the power to retain its juices, but to throw or eject them on the stigma; which I have repeatedly seen it do on a warm sunny day. I have before observed how uncommonly perfect the mechanism is in most flowers in very hot weather, when the spiral wire seems full of vigour, and all its various offices are shown with double force. It is on such a day the liquid of the pistil melts the pollen with more ease; for in general the drop appears on the stigma but one hour, and then retires. This also is the mechanism of the pistil; the curious motion in the nectary of the *ranunculus* is known to a few; the leaf, which is formed to cover it, always encloses it tight if the wind blows, but, on the contrary, admits the rays of the sun to it, if it is a *fine warm day*. There cannot be a more excellent barometer, for it will denote each cloud and each change of the atmosphere; and there are many flowers which draw on, and put off their cover, whenever a threatening cloud appears. In watching flowers very exactly, it is really a perpetual source of astonishment—the varying mechanism is so great, that no person who would take the trouble of sitting by a plant for a few hours, could ever after admit a doubt of its being governed by the mechanical powers.

Mechanical motion of the nectary.

Your humble Servant,
AGNES IBBETSON.

III.

Chemical Researches on the Blood, and some other animal Fluids. By W. T. Brande, Esq. F. R. S. *Communicated to the Society for the Improvement of animal Chemistry, and by them to the Royal Society.*

(Concluded from p. 32.)

Researches on the colouring matter of the blood.

1. TO procure this substance for experiments, I generally employed venous blood, which had been stirred during

Method of collecting the colouring matter of blood.

its coagulation; the fibrina is thus removed, and the colouring matter diffused through the serum, from which it gradually subsides, being difficultly soluble in that fluid; on decanting off the supernatant serum, the colouring matter remains in a very concentrated form. When other modes of procuring it were employed, they will be particularly mentioned; but as I have not found the serum which is retained interfere much with the effects of various agents upon the colouring principle, the method just noticed was commonly adopted.

It appears to consist of globules,

not soluble in water,

2. When the colouring matter thus collected is microscopically examined, it seems, as Lewenhoeck first observed[*], to consist of minute globules. These are usually described as soluble in water, a circumstance which my own observations led me to doubt, and which the more accurate experiments of Dr. Young, an account of which, intended for publication, he has kindly permitted me to peruse, have completely disproved.

though the colouring principle in them is.

3. The effect of water upon the red globules is to dissolve their colouring matter, the globule itself remaining colourless, and, according to Dr. Young, floating upon the surface.

The aqueous solution decomposed by heat,

This aqueous solution is of a bright red colour, and not very prone to putrefaction. When heated, it remains unaltered at temperatures below 190° or 200° Fahrenheit; at higher temperatures it becomes turbid, and deposits a pale brown sediment: if in this state it be poured upon a filter, the water passes through without colour, so that exposure to heat not only destroys the red tint, but renders the colouring matter insoluble in water.

alcohol, and ether.

Alcohol and sulphuric ether added to this solution also render it turbid, and when these mixtures were filtrated, a colourless and transparent liquor was obtained.

The precipitate partly soluble in acids.

4. The matter remaining upon the filter was insoluble in water, in alcohol, and in sulphuric ether; but when digested in dilute muriatic or sulphuric acid, a portion was taken up forming a brown solution. I regard this soluble portion as a modification of the colouring matter produced by the operation of heat: the insoluble residuum had the properties of albumen.

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[*] Haller, Elem. Physiolog. Vol. 1, p. 51.

5. *Effects of Acids on the colouring Matter.*

A. Muriatic acid, poured upon the colouring matter of the blood, renders one portion of it nearly insoluble, and of a bright brown colour : another portion is taken up by the acid, forming a dark crimson solution when viewed by reflected light ; but when examined by transmitted light, it has a greenish hue.

The colouring matter treated with muriatic acid :

This solution remains transparent, and its colour is unimpaired by long exposure to light, either in contact with the air, or when kept in close vessels. At its boiling temperature the colour is also permanent.

Infusion of galls produces no change in this muriatic solution, nor is its colour affected by carbonated alkalis, even when added in considerable excess.

It is rendered brown red by supersaturation with caustic potash, but not with soda, or ammonia : these, and especially the latter, rather heighten its colour.

When considerably diluted with water its original colour is much impaired, and the green hue, which it always exhibits by transmitted light, becomes more evident.

In preparing this solution, I frequently employed the coagulum of blood cut into pieces, and digested in equal parts of muriatic acid and water, at a temperature between 150° and 200°. In three or four hours the acid was poured off, and filtrated. The clear solution was in all respects similar to that above described, although before filtration it appears of a dirty brown colour.

I evaporated a portion of this muriatic solution in a water-bath, to dryness ; it retained its colour to the last, and left a transparent pellicle upon the evaporating basin, of a dirty red colour : this, when redissolved in muriatic acid, acquired its former tint, but the colour of its aqueous solution was nearer brown than red.

B. Sulphuric acid, diluted with eight or ten parts of water, forms an excellent solvent of the colouring principle of the blood.

with sulphuric :

It may be employed in a more concentrated state, but the bright colour of the solution is in this case apt to be impaired ; and when more largely diluted with water, its action is slow and uncertain. Either the sediment of the colouring matter from the serum, or the crassamentum of the blood, may be indifferently employed in forming these solutions.

When dilute sulphuric acid is added to the colouring matter, it renders it slightly purple; and, if no heat be applied, the acid, when poured off and filtered, is colourless; so that dilute sulphuric acid, when cold, does not dissolve this colouring principle.

One part of the crassamentum of blood cut into pieces was put into a matrass placed in a sand heat, with about three parts of dilute sulphuric acid. It was kept for twelve hours in a temperature never exceeding 212°, nor below 100°. After twenty-four hours the acid was filtered off, and it exhibited a beautiful bright lilac colour, not very intense, and tainted with green when viewed by transmitted light.

This solution is nearly as permanent as that in the muriatic acid. Some of it, which has been kept for a month in an open vessel, often exposed to the direct rays of the sun, is very little altered.

When diluted with two or three times its bulk of water, the lilac tint disappears, and the mixture is only slightly green.

When exposed to heat, the colour gradually changes as the acid becomes more concentrated by evaporation, and when reduced to about half its bulk, the lilac hue is destroyed.

The solutions of pure and carbonated alkalis, when added in excess, convert the colour of this sulphuric solution to brownish red; but in smaller quantities they merely impair it by dilution.

with nitric: C. Nitric acid, even much diluted, is inimical to the colouring matter of the blood.

A few drops added to the muriatic or sulphuric solutions gradually convert their colour to a bright brown, and larger quantities produce the same change immediately.

The action which this acid exerts upon the colouring matter under other circumstances is nearly similar, and always attended with its decomposition, so that my attempts to procure a red solution in this menstruum uniformly failed of success.

with acetic: D. Acetic acid dissolves a considerable quantity of the colouring matter of the blood; the solution is of a deep cherry red colour. When somewhat diluted, or when observed in tubes of about a quarter of an inch bore, this solution appears perfectly green by transmitted light. In its other habitudes it nearly resembles the muriatic solution.

with oxalic, citric, and tartaric.

E. The solution of the colouring matter in oxalic acid is of a brighter red than those hitherto noticed; that in citric acid is very similar to the acetic solution, and with tartaric acid the compound somewhat inclines to scarlet. All these solutions exhibit the green hue, to which I have so often alluded, in a remarkable degree.

6. *Effects of Alkalis on the colouring Principle of the Blood.*

Action of the alkalis on the colouring principle.

The caustic and the carbonated alkalis form deep red solutions of this substance, which are extremely permanent.

1. Solutions of pure potash, and of the subcarbonate, take up a large proportion of the colouring matter of the blood. The intensity of the colour of this solution, when concentrated, is such, that it appears opaque, unless viewed in small masses, or in a diluted state, when it is of a bright red colour.

2. In soda and its subcarbonate the solution has more of a crimson hue, which colour is extremely bright in its concentrated state.

3. The solution in liquid ammonia approaches nearer to scarlet than that in which the fixed alkalis are employed.

4. When these alkaline solutions are supersaturated with muriatic acid, or with dilute sulphuric acid, they acquire a colour nearly similar to the original solutions in those acids, which have been above described.

5. Nitric acid, added in small quantities, or even to saturation of the alkaline menstruum, heightens the colour of the three compounds; but when there is a slight excess, a tint of orange is produced, which soon passes into bright yellow.

6. The alkaline solutions may be evaporated nearly to dryness without losing their red colour; during the evaporation of the ammoniacal solution, the alkali flies off, and a brown red solution of the colouring matter in water remains.

Examination of the affinities of the colouring matter.

Having ascertained the above facts respecting the colouring principle of the blood, I next proceeded to examine how far it was susceptible of entering into those combinations which are peculiar to other varieties of colouring matter.

These experiments I shall detail in the order in which they were made.

1. Some

Alumine does not form a permanent red with it.

1. Some pure alumine was added to a concentrated aqueous solution of the colouring matter of the blood, and after twenty-four hours, the mixture, which had been frequently agitated during that period, was poured upon a filter, and the residuum washed with hot distilled water.

The filtrated liquor had lost much of its original colour; the alumine had acquired a red tinge; it was dried at a temperature between 70° and 80°, during which it became brown.

2. Two hundred grains of alum were dissolved in four fluid ounces of a solution of the colouring matter, similar to that employed in the last experiment. The colour of the compound was bright red. Liquid ammonia was added, and the precipitate collected, and carefully dried. It was of a dirty red, and after some days exposure to light, became nearly brown.

From these, and other experiments, which I have not thought it necessary to detail, it appears that alumine will not form a permanent red compound with the colouring principle of the blood; I was therefore next induced to employ oxide of tin.

Muriate of tin combined with it,

3. Fifty grains of crystallized muriate of tin (prepared by boiling tin filings in muriatic acid, and evaporating the solution) were dissolved in four ounces of the solution of colouring matter, which immediately assumed a purple tint, and became afterward brown. It was diluted with twice its bulk of water, and put aside in a stopped phial. On examining it three days afterward, a small quantity of a bright red powder was observed at the bottom of the phial, which proved to consist of the colouring principle combined with the metallic oxide. A portion of this compound, which has been kept in water for some weeks, has undergone no change of colour; but when dried by exposure to air, it loses its brilliant tint, and becomes of a dull red hue.

and the acid separated.

To a compound solution of muriate of tin and colouring matter, similar to that employed in the last experiment, I added a sufficient quantity of solution of potash to decompose the salt of tin. The precipitate thus obtained was collected, and dried by exposure to the air of a warm room. It was of a dull red colour, and has undergone no apparent change by exposure to the joint action of light and air for three weeks.

4. Find-

Attempt to combine it with supertartrate of potash, and oxide of tin.

4. Finding that supertartrate of potash exalted the colour of the blood, I endeavoured to form a compound of it with that substance and oxide of tin, and thus, in some measure, to imitate the process in which cochineal is employed for the production of scarlet dye; but although a bright red compound is produced, when it is dried at a very moderate temperature its colour becomes similar to that of the other combinations which I have described.

The experiments varied,

These experiments I repeated in various ways, occasionally applying the salt of tin as a mordant to woollen cloth, linen, &c.; but the brilliancy of the colour was never permanent.

Oak bark used as a mordant.

5. Having observed that infusion of galls and decoction of oak bark do not impair the colour of the blood, I conceived that solution of tannin might answer the purpose of a mordant, as it is effectually employed by dyers in giving permanence to some of their red colours.

I accordingly impregnated a piece of calico with decoction of oak bark, and afterward passed it through an aqueous solution of the colouring matter of blood. When dry, it was of a dirty red colour, nearly similar to that which would have been obtained, had no mordant been applied: when, however, an alkaline solution of the colouring matter was employed, the colour was equal to that of a common madder red, and as far as I have been able to ascertain, it is permanent.

Oxide of lead.

6. A solution of superacetite of lead was impregnated with the colouring matter of the blood. The compound was bright red: no spontaneous change took place in it, and on the addition of an alkali a white precipitate was formed, the fluid retaining its former tint.

From this, and other experiments, in which it was attempted to combine oxide of lead with the colouring matter of the blood, it would appear, that there is no attraction between these two substances.

Solution of mercury.

7. The most effectual mordants, which I have discovered for this colouring matter, are some of the solutions of mercury, especially the nitrate, and corrosive sublimate.

Nitrate of the red oxide, and

Ten grains of nitrate of mercury, (prepared with heat, and containing the red oxide) were dissolved in two fluid ounces of a solution of the colouring matter of the blood. After some hours a deep red compound was deposited, consisting chiefly of the metallic oxide combined with the colouring matter, and a small portion of coagulated albumen. The remaining fluid had nearly lost its red colour.

of the black.

The nitrate of mercury containing the black oxide produces nearly similar effects, excepting that the colour of the compound is of a lighter red.

Corrosive sublimate.

When corrosive sublimate is added to the solution of the colouring matter, its tint is instantaneously brightened, and it becomes slightly turbid from the deposition of albumen. If this be immediately separated by a filter, the liquor which passes through gradually deposits a deep red or purplish insoluble precipitate, and if it now be again filtrated the liquid is colourless, the whole of the colouring principle being retained in the compound which remains upon the filter.

These salts used as mordants.

By impregnating some pieces of woollen cloth with solution of nitrate of mercury, or of corrosive sublimate, and afterward steeping them in an aqueous solution of the colouring matter of the blood, I succeeded in giving them a permanent red tinge, unalterable by washing with soap; and by employing the ammoniacal solution of the colouring matter, calico and linen may be dyed with the same mordant.

In these experiments I was much satisfied by the complete separation of the colouring matter from its solutions, which after the process were perfectly colourless.

VII. Some Remarks on the preceding experimental Details.

Analogy between coagulum of chyle and curd of milk.

From the experiments related in the second section of this paper, it appears, that sulphuric acid effects changes upon the coagulum of chyle, similar to those which Mr. Hatchett has observed to result from the action of dilute nitric acid upon the coagulated white of egg. This last substance, however, is not convertible into gelatine by means of sulphuric acid, whereas in these respects the curd of milk resembles that of chyle: this circumstance, as well as the more ready solubility of the coagulum of chyle in dilute, than in concentrated acids, points out a strong analogy between these two bodies.

Sweet taste of chyle.

The sweet taste of chyle naturally suggested the idea of its containing sugar*; but I am not aware of any direct experiments

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* Fordyce on Digestion, 2d Edition, p. 121.

ments which have demonstrated its existence, and have therefore detailed minutely such researches as I have been enabled to make upon the subject, hoping at some future period to render them more complete.

No gelatine in blood.

The experiments to prove the nonexistence of gelatine in the serum of blood will, I trust, be deemed sufficiently decisive: they show, that that abundant proximate principle of animals is not merely separated from the blood, in which it has been supposed to exist ready formed, but that it is an actual product of secretion.

It is a product of secretion.

The blood contains but little iron, and this not peculiar to the red part.

The proportion of iron afforded by the incineration of several varieties of animal coal is much less considerable than we have been led to expect, and the experiments noticed in the fifth section show, that it is not more abundant in the colouring matter of the blood, than in the other substances which were submitted to examination; and that traces of it may be discovered in the chyle, which is white, in the serum, and in the washed crassamentum or pure fibrina.

The colouring principle of a peculiar nature;

The inferences to which I have alluded in the first section of this paper are strongly sanctioned by these facts, and coincide with the opinion, which has been laid before the Royal Society by Dr. Wells*, respecting the *peculiar nature* of the colouring principle of the blood, and support the arguments which are there adduced.

independent of iron: and probably may be useful in dying.

That the colouring matter of the blood is perfectly independent of iron, is, I conceive, sufficiently evident from its general chemical habitudes, and it appears probable that it may prove more useful in the art of dying, than has hitherto been imagined, since neither the alkalis nor the acids (with the exception of the nitric) have much tendency to alter its hue. The readiness, too, with which its stains are removed from substances to which no mordant has been applied, seem to render it peculiarly fit for the purposes of the calico-printer. I have not extended these experiments, nor have I had them repeated on a sufficient scale to enable me to draw more general conclusions respecting the possibility of applying them with advantage in the arts: this would have led me into too wide a field, and one not immediately connected with the objects of this society: the subject, however, appears important.

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* Phil. Trans. 1797.

So employed by the Armenians.

It is not a little remarkable, that blood is used by the Armenian dyers, together with madder, in the preparation of their finest and most durable reds*, and that it has even been found a necessary addition to insure the permanency of the colour†. This fact alone may be regarded as demonstrating the non-existence of iron as the colouring principle of the blood, for the compounds of that metal convert the red madder to gray and black.

Menstrual fluid.

While engaged in examining the colouring matter of the blood, I received from Mr. William Money, house surgeon to the general hospital at Northampton, some menstruous discharge, collected from a woman with prolapsus uteri, and consequently perfectly free from admixture of other secretions. It had the properties of a very concentrated solution of the colouring matter of the blood in a diluted serum, and afforded an excellent opportunity of corroborating the facts respecting this principle, which have been detailed in the preceding pages. Although I could detect no traces of iron, by the usual modes of analysis, minute portions of that metal may, and probably do exist in it, as well as in the other animal fluids which I have examined; but the abundance of colouring matter in this secretion should have afforded a proportional quantity of iron, did any connection exist between them. It has been observed, that the artificial solutions of the colouring matter of the blood, invariably exhibit a green tint when viewed by transmitted light: this peculiarity is remarkably distinct in the menstruous discharge‡.

Rapid reproduction of perfect blood.

I hope that some of the facts furnished by the above experiments may prove useful to the physiological inquirer: they account for the rapid reproduction of perfect blood after very copious bleedings, which is quite inexplicable upon that hypothesis which regards iron as the colouring matter, and may

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* Tooke's Russian Empire, Vol. III, p. 497.

† Aikin's Dictionary, Art. Dying, and Philos. Magazine, Vol. XVIII.

‡ I could discover no globules in this fluid; and although a very slight degree of putrefaction had commenced in it, yet the globules observed in the blood would not have been destroyed by so trifling a change.

perhaps

Formation of the colouring matter.

perhaps lead to the solution of some hitherto unexplained phenomena connected with the function of respiration. There can, I think, be little doubt, that the formation of the colouring matter of the blood is connected with the removal of a portion of carbon and hidrogen from that fluid, and that its various tints are dependent upon such modifications of animal matter, and not, as some have assumed, upon the different states of oxidizement of the iron which it has been supposed to contain.

IV.

Remarkable Effects of the spontaneous Rise and Overflow of heated Soap Lie in a metallic Pump. In a Letter from R. B. with Remarks by W. N.

To Mr. Nicholson.

SIR,

Index to the Journal.

I WAS much pleased at the sight of your advertisement on the wrapper of your last number, in which you promise an extensive Index to the whole Journal. I have sent my subscription to the publisher; and, as an old correspondent, who has been your disciple from the very first, I think it incumbent on me to look round for a few philosophical facts, and give my assistance to your work as far as my ability may extend. I wonder that you, who are known to be so intimately acquainted with the practice of all our manufactures, have never given us a detail of what may be called the philosophy of a workshop. In the daily performance of manipulations, and the adoption of expedients to ensure success, the manufacturer, by the chance of events, and under the necessity of making repeated trials, is continually, and for the most part unconsciously, availing himself of the tenacities, the toughness, the brittleness, the excitement and conducting of heat, the production and condensation of elastic fluids, the statical and hydrostatical powers, with the chemical energies of bodies, in such a variety of ways, as would, and often does, afford the most certain instruction to theorists, and not unfrequently the means of improving, in return, the very art under contemplation. I write these reflections as they flow from my pen, and as a kind of preface to a fact

Philosophy of a workshop.

Introductory remarks.

fact with which I find myself embarrassed. You have often obliged your correspondents with disquisitions or investigations in cases like the present. Whether this may lead to any useful result beyond the mere fact, I know not: I am sure you will admit it to be curious; and if you have not seen it, I beg you will take the trouble to do so, and oblige me, and your other readers, with your opinion of its cause.

Description of a boiler containg soap and lie:

Some time ago I visited an extensive soap manufactory, in which there were several large boilers, each capable of holding a tun of soap. One of them was in a state to have the lie drawn off from beneath the soap, which formed a fluid mass between three and four inches thick, above a depth of perhaps four feet of lie. I speak from memory throughout. There was a wooden back or vessel on one side to receive the lie, and a copper pump was lowered down into the boiler from a situation in which it had hung suspended above by a tackle. I think the barrel of the pump might be about four inches in diameter, terminating in a small copper cistern at top, with a spout of about three inches bore, proceeding from one side of the cistern close to its bottom. I do not remember whether the pump was primed or not, but I think not, and suppose it may have had metallic valves, and perhaps a hempen packing, instead of leather, which could not be used with soap lie. The pump was, I doubt not, lowered so as to rest on the bottom of the boiler, for it could not else have been steady—and a workman began to pump. The lie came out of the spout in a stream which did not half fill the bore, and it fell at no considerable distance from the spout. During the whole experiment the soap and the lie in the boiler appeared level and motionless, neither circulating nor showing any other sign of its high temperature, the soap at top preventing even the steam from appearing. But after a very few strokes the lie dashed out of the spout with a sudden noise, and flew to the opposite side of the receptacle in a stream hot, smoking, frothy, and filling the bore of the spout. The workman left off pumping, and the stream continued with unceasing violence and rapidity. I was astonished, and stood gazing at this striking effect, my mind being engaged with a mixture of wonder at the phenomenon and puzzle at its continuing, when the apparent cause had ceased to act. But after a considerable time, I turned to the assistants, and asked, "how it was

and a copper pump to draw off the lie from beneath the soap.

When the hot lie was first pumped, the effect was not remarkable,

but after a few strokes it flew out spontaneously,

and continued

was to be stopped?" Three men immediately came with buckets of cold water, which they dashed against the pump, and the stream immediately slackened, but did not quite cease. It was recovering strength, and I have no doubt would have arisen to its former violence if a fourth bucket of water had not been brought in time, and dashed against the pump, upon which it entirely ceased. These are the facts; to which I will only add, that I think the immersed part of the pump was between three and four feet, and the spout might have been between two and three feet above the surface of the fluid in the boiler. Why hot soap lie, in these or any other circumstances, should spontaneously rise and run with violence out of an aperture more than two feet above its quiet level, is an event upon which I cannot make even a probable conjecture.

till cold water was thrown against the pump.

I am, Sir,
Your obliged Reader,
R. B.

Reply. W. N.

Some years ago, an application was made to me to report professionally upon a pump, which was described to me; and it was added, that it produced the effect of raising water to a greater height, by what is called suction, than 33 or 34 feet. My report was, that the pump was a bad one, and that it was incapable of producing the pretended effect. Some time afterward, however, the same person called on me, and said, that the parties who had commissioned him to apply to me had been deceived by the inventor, who had made a small hole in the suction-pipe. This led to observation on my part, that my general conclusion upon the merits of the pump would have been the same, if this fact had been stated along with the others; but that it would have been accompanied with an indication of the time and place where that trick had been played a century before; which does in truth, by mixing air with the column of water, enable it, (because upon the whole lighter,) to rise to a greater height than mere water; but that the effect, taking height and quantity into the consideration, would be less than that of a common pump.

Account of a pump which raised water by suction higher than 34 feet.

pump. I relate this, not because I infer the existence of any trick in the remarkable fact so clearly and fully described by my correspondent, but because it appears to indicate the solution. I have not, however, thought the analogy so perfect as to preclude the necessity of visiting a soapwork, and of making an experiment which seems altogether to clear up the subject.

The facts at the soap manufactory were precisely as he describes them; and very striking indeed, notwithstanding my expectation having been previously raised by R. B.'s description. The course of reasoning suggested by them were as follows:

Inferences respecting the cause of the continued rise and flow of the heated lie. Steam was first produced under the partial vacuum of the pump, and continued from the thin space between the bottom of the suction-pipe, and of the boiler. This rendered the column in the pump lighter, and caused its rise to continue.

The lie and melted soap upon the surface, though strongly heated, were not sufficiently so to cause steam to be produced under the pressure of four feet of fluid, added to that of the atmosphere. But, when a part of this last pressure was taken off by the action of the pump, the fluid in contact with the bottom, and immediately under the suction-pipe, was enabled to give out steam; and the thin portion of fluid lying between the metallic edge of the pipe, and the bottom of the boiler, would be more particularly so disposed (it being a well-known fact, that such thin films are very readily converted into steam, as is strikingly shown by heating water in a glass vessel with a small metallic ball or piece of glass lying in it; most of the bubbles seeming to spring from these small bodies.) The progressive increase of steam will render the fluid in the pump-barrel frothy and lighter than the dense lie in the boiler; and as soon as the reaction of its specific gravity becomes so much as that a column of the whole length of the pump shall be lighter than a column of the dense lie, of no greater length than answers to the depth of the boiler, this last will predominate, and cause the other to ascend without the aid of the pump. Now the first developement of the steam was effected by the action of the pump, in taking off atmospheric pressure; but as soon as the pump became very hot, and the steam very copious, the pressure of the inclosed column may be conceived to have become much less than is requisite to allow the continued developement of steam; and the column being allowed to flow out sideways, instead of ever rising to a counterpoise, the diminished pressure, and also the afflux of the hot frothy lie, becomes permanent. And the velocity of emission will be governed by the height to which the lie is enabled or allowed to rise in the trough above the level of the spout. I am greatly disposed to think the condition that the heated lie must all pass between the two hot metallic bodies, the lower edge of the suction-pipe, and the bottom of the boiler, is of great consequence to the whole effect.

Experiment. Water boiled in a vessel, in which an upright glass tube was placed, stood higher in the tube.

In order to bring the subject in some measure to the test of experiment, I took a tubular glass vessel, ten inches long, and one inch internal diameter, having a small enlargement, or foot to stand upon, as a chemical measure. By a smart stroke with the end of a pointed file, I drove the bottom in, by making a large hole in the middle without breaking the other parts. In this state it was capable of standing on its bottom as before. A tin vessel with a flat bottom was then filled to the depth of three inches with water, and the glass vessel being set upright in the water, the whole was placed on the fire. As soon as the liquid began to boil, the bubbles were most plentiful in the tube, and the water stood at rather less than three quarters of an inch higher in the tube than in the exterior vessel. When the boiling was very rapid, the difference was rather less, and when the tube was raised a little from touching the bottom, the difference ceased. Upon taking the tube out, and letting it cool, and then setting it as before in the boiling water, the difference or rise did not take place, nor was any boiling seen within the tube till after the lapse of about half a minute; and upon inverting the tube, so that its mouth touched the bottom of the tin vessel, and the foot was uppermost, the water within it did not stand so high, nor boil so fast, as when the contrary position was adopted. And lastly, the difference appeared rather more considerable when the boiling was moderate, than when the whole mass of the fluid was full of steam-bubbles by rapid ebullition.

Particular circumstances of the experiment.

Inferences. The higher station in level of the water in the tube was occasioned by steam produced most plentifully between the lower part of the tube, and bottom of the vessel.

Though the preceding experiment is not so apposite and conclusive as it might have been made, by using a greater depth of a saline fluid, and allowing a side aperture for the same to flow out as in the pump; yet it appears to me to show with sufficient precision, 1. That the column of fluid in the tube was of less specific gravity, from the greater admixture of steam-bubbles, than a like column of the exterior heated fluid, and therefore stood higher. 2. That the excessive quantity of steam was produced from the thin film of water between the foot of the tube, and bottom of the tin vessel; and that less steam being produced when the mouth of the tube was downwards, and the film less extensive, the internal fluid did not stand so high. 3. That it would have stood higher, if there had been a side aperture; because this would have caused a rapid access of new water to the film, and carried all the bubbles inwards. 4. That upon raising the tube, the film lost its thinness, and the effect ceased. 5. That the effect was less when the exterior fluid was rendered lighter by strong boiling, which filled it with steam-bubbles. 6. And that the cold tube produced no effect, but required to be first heated to the boiling point, before any steam could be produced in contact with it.

V.

On the Combination of Chlorine with Oil of Turpentine. In a Letter from Mr. R. PORRETT, *jun.*

To Mr. Nicholson.

SIR, *Tower, Oct.* 2, 1812.

Reference to Mr. J. Davy's paper. Liq. Libav. and oil of turpentine.

I HAVE just read Mr. John Davy's paper, published on the 1st of last month, in No. 151 of your Journal, on the combination of chlorine; and my attention was particularly engaged by that part of it in which he mentions an experiment of his on the action of Libavius's liquid, or stannanea, on oil of turpentine, and states, that the oil became viscid, less volatile than before; that it had little taste or smell; and that its solution in alcohol, when dropped into water, occasioned a cloudiness in that fluid.

Analogous experiments formerly made by the author.

This experiment of Mr. J. Davy's recalled to my mind some analogous experiments which I made long before his, and in which I formed, as I have some reason for believing, a similar fluid to that obtained by Mr. J. Davy; and as that gentleman expresses a wish, that a subject so curious might engage the attention of chemists, I think this a fit opportunity for requesting your insertion of those experiments in your valuable Journal. Imperfect as they are, they may thus afford some information, and will become more generally known than they can be by the communications of them which I have made in conversation with my chemical friends, and in a lecture on chlorine and muriatic acid, which I delivered to the Mathematical Society on the 22d of February last.

Date of the experiments.

To avoid the suspicion, that any part of what I have now to communicate is borrowed from Mr. J. Davy's paper, I think it best to transcribe, verbatim, my memorandum of the first experiment, in which I combined chlorine with oil of turpentine. It was performed some time between the 24th of July, and 1st of November, 1808, those being the dates of the experiments immediately preceding and following that which I am about to transcribe, and which in my memorandum I had omitted to date. The following is the copy of the memorandum.

"*Experiment with Oil of Turpentine.*

Oxymur. gas and oil of turpentine in the state of vapour. A heavy, thick oil was formed, &c.

"Wishing to ascertain the effect of oxymuriatic gas on oil of "turpentine, I caused the vapour of the latter and the oxymu- "riatic gas to pass together through a glass tube conducting into "a glass receiver. There was formed by this process a very "heavy white oil, which sunk immediately in water, was as "thick as otta of roses; smelt and tasted very much like nut- "megs, but communicated rather a more caustic sensation to "the tongue. I did not observe any disengagement of gas, "arising from the chemical combination that was going on; "very little was expelled from the receiver, and that, I believe, "was only oxymuriatic. I did not collect and examine it, to "ascertain whether any other gas was present, but the smell "proved that that gas was evolved; and as it only took place "when the extrication of the oxymuriatic acid from the mate- "rials that produced it was violent, there is some reason for "thinking it was not contaminated with any other gas, which "might be supposed to be formed from the oil of turpentine. "The change produced on the oil of turpentine in this experi- "ment is very great, from an exceedingly light and thin fluid "to a very heavy and viscid one, differing likewise totally in "its smell and taste."

Repetition of the experiment.

I have since formed a similar fluid by passing a large quantity of chlorine gas through a small quantity of oil of turpentine. The product of this experiment I exhibited to the Mathematical Society during the lecture before mentioned; and I have a

little

little now remaining in my possession. Its taste is the same as of that obtained in the first experiment; viz. a bitter aromatic, uniting the flavour of hops with that of nutmegs. I have remarked, that the flame arising from its combustion is edged with green.

Compound of the fluid with sulphur.

Still more recently, viz. on the 15th of January last, I obtained a compound of this fluid with sulphur, by acting on oil of turpentine by Dr. Thompson's sulphuretted liquid, or sulphurane. This compound is decomposed by solution of potassa, which dissolves the sulphur, and separates the peculiar fluid, which then exhibits, as far as I have been able to judge, all the characters of that formed by the direct combination of chlorine with oil of turpentine. This experiment, I recollect, appeared to me, at the time that I made it, as very favourable to the opinion formed by Sir Humphrey Davy of the nature of Dr. Thompson's sulphuretted liquid.

Whether this peculiar fluid be the same as that of Mr. John Davy.

I cannot positively say whether the peculiar fluid formed in the three experiments just described is identical with that produced by Mr. John Davy from the mutual action of stannanea and oil of turpentine. Reasoning from analogy would induce me to conclude that it is; but some differences in our descriptions of those fluids oblige me to suspend forming a decided opinion, until the fluids themselves have been compared.

I am, Sir,
Your most obedient Servant,
R. PORRETT, Jun.

VI.

On the Electrical Effects produced by Friction between Bodies. In a Letter from J. A. De Luc, *Esq. F. R. S.*

To William Nicholson, Esq.

SIR,

Reference to former papers.

IN my former papers I have communicated to you my remarks on Dr. Maycock's electrical system; and I come now to his paper in your Journal, No. 144, concerning the *production of electrical excitement by friction*. This paper concludes by the following very judicious remark, which induces me to offer here to him my ideas on the same subject.

"It will afford me much pleasure (says Dr. Maycock) "should these observations call the attention of your readers to "the *theory* of *electrical excitement.* I trust that, while we are "successfully employing the powers of electricity in chemical "analysis, we shall not altogether neglect to investigate the "means by which these powers are called forth, and the laws "by which their action is regulated. It has, with much in-"justice, been objected to *theoretical* pursuits, that they lead "to none of the *practical* advantages, which interest the happi-"ness of society. The remark is indeed true, if applied to "particular discoveries; but these are to be considered only as "the elements from which physical science first took its ori-"gin, and by which it is daily nourished and supported. Let "it never be forgotten, that our most perfect instruments, "those which promote no less our comfort, than they tend to "advance our intellectual improvement, are the invaluable "fruits of philosophy." Journ. vol. XXXI, p. 309.

Theory of electrical excitement deserves attention.

1. In quoting this passage with approbation, I cannot, Sir, but express again my regret, that Dr. Maycock appears to have no knowledge of my papers in your Journal; for they might have given him the opportunity of useful examinations between us. For instance, in your No. 126, for January, 1811*, is my paper under the title of *Experiments, showing the effects of Friction between bodies*; which experiments might have afforded him what he wishes to find in your readers, viz. some *remarks* to be compared with his *theory.* But if he reads my present paper, there will be only a little time lost, and the examination may now be effected more directly between us in your Journal.

Effects of friction before examined.

2. Dr. Maycock's system on the effects of *friction* is derived from his opinion, which, in my former papers, I have proved to be unfounded, viz. that the *electrical* effects produced by the association of *two* proper *metals* appeared only when they came to be *separated.* Had Dr. Maycock known these papers, he certainly would have thought it proper to answer me, before he took his system as a principle in explaining the effects of *friction,* as he does thus in vol. XXXI, p. 305. "It "must be obvious, that, while we are drawing one body over another,

Dr. Maycock's opinion of these effects.

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* Vol. XXVIII, p. 1.

" another, a number of points in the surface of the *rubber* are " first brought into *contact* with a corresponding set of points " in the surface of the *body rubbed*; that they are then *separated* from them, and brought into *contact* with another set of " points; and so on, until the one body has passed entirely over " the other. Now, at each *separation*, if the bodies be of different " kinds, whether conductors or nonconductors, the general " law, we have stated, must operate. and opposite *electrical* " *states* must be excited in the *separated* particles. So far, " therefore, the *excitement* by *friction*, and the *excitement* by " *contact* and *separation*, appear to be referrible, in a general " manner, to the same principle. We shall now proceed to a " more particular *consideration* of the subject."

This theory difficult to prove.

3. To this *consideration* I shall soon come; but I must first observe, that it would be very difficult to prove that theory by ascertaining the effects of the *friction* in different points of the *rubber* and the *body rubbed*, in order to find out their *progress*. We see, upon the *whole*, that one is become *negative*, when the other is made *positive*; but nothing can indicate whether these effects are produced during the *contact*, or only at the *separation*. Therefore the decision of this point must proceed from other phenomena, and Dr. Maycock affords me an opportunity of discussing this point by the passage which follows that above quoted.

Principal facts relative to excitement by friction, according to him.

" The principal facts (he says) relative to the *excitement* of " bodies by *friction*, may be expressed by the five following " propositions. 1. To produce *excitement* by friction, it " is essentially necessary that one of the bodies employed in " the operation be of the class of *electrics*. 2. If two *electrics*, " or an *electric* and an *insulated conductor* be employed, the one " body will, after the operation, indicate an electricity opposite " to that which is indicated by the other. 3. The effect of " *friction* performed with one combination of *dissimilar* bo" dies is different from that which is produced by any other " combination. 4. The *friction* of two bodies, *similar* in all " respects to one another, produces *no excitement*. 5. If the " *rubber* of an *electrical machine* be insulated, only a very " slight charge can be accumulated in the prime conductor; " and, under such circumstances, the action of the machine " soon ceases altogether."

Errour in the distinction between electrics and conductors, all bodies conducting more or less: but on some the effects are not propagated.

5. I shall first observe, that, had Dr. Maycock read my paper on the effects of *friction*, to which I shall here refer on many points, he would have seen the errour of the first electricians in their distinction of bodies, which he continues to admit, that of *electrics* opposed to *conductors*; as if the former only had the faculty to be *electrified* by *friction*. With respect to *electricity*, there is no other distinction than that of more or less *conductors*, which explains all the phenomena. From the property of *absolute nonconductors*, as are *resinous* bodies, whatever change is produced in the *electrical* state of their surface, either by *friction*, or by *communication* with an *electrified* body, it is not *propagated* on them; and this is their only distinctive property with respect to *electrical* phenomena. The difference, therefore, between these bodies, and the *imperfect nonconductors*, is this; that the changes produced on some points of the latter, either by *friction*, or by *communication* with an *electrified* body, are propagated on their surface, slowly on some, as *glass*, or almost instantly on the *best conductors*, such as *metals*.

Motion of the electric fluid along different bodies.

6. From this determination of the effects of the different *conducting* faculties of bodies, united with that of the nature of the electric fluid, which Dr. Maycock has not thought necessary to investigate, I derived in the same paper (pp. 3 and 4) the following theory of the effects of *friction*, which is to be compared with the phenomena. "The *electric fluid* resides "on all terrestrial bodies, every particle of *air* included; being "retained upon them by a mutual attraction, which, however, "differs in degree; some attract the *electric fluid* only when it "comes into *contact* with them; but then it *adheres* strongly "to the parts which receive it, or moves but very slowly along "their surface; which therefore are *nonconductors*: others "receive it at more or less *distance*, and it is *propagated* more "or less rapidly along their surface. *Glass*, though absolutely "*impermeable* to the *electric fluid*, permits it to move with a "sensible progress along its surface."

Theory of the effects of friction.

7. After these definitions of the *nature* of the *electric fluid*, and of its *motions* along different bodies, I thus define the effects of *friction*, connected with these premises. "*Friction* "excited between two bodies, has no other effect than that of "*disturbing* the natural *equilibrium* of the *electric fluid*, which "*equilibrium* tends always to be produced among all bodies,

" according to its actual, but local (in a certain extent) quanti- " ties on them, and in the ambient *air*. If both the bodies which " exercise *friction* on each other are *good conductors*, the *equi-* " *librium* being constantly restored, this disturbance is not " perceived: but if one has more disposition than the other to " attract the *electric fluid* thus agitated, with the faculty of " transmitting it to its remote parts; when the bodies are " separated, either suddenly, or in general before the *equili-* " *brium* of the *electric fluid* is restored between them, one is " found *positive*, as having acquired a proportional quantity of " this *fluid*, greater than the *ambient air*, and the other *negative*, " as having lost that quantity." This is the theory of the effects of *friction*, which, in the same paper, I compare with direct experiments: but before I come to that comparison, I must explain the general plan of those experiments, and its motive.

Motive and plan of Mr. De Luc's experiments.

8. The obscurity which reigned on the effects of *friction* proceeded from a circumstance wanting in most of these experiments; they require the *insulation*, not of *one* only of the bodies, but of *both*, either *conductor* or *nonconductor*; else the whole of the reciprocal effect cannot be discovered. I had found this necessity by many experiments made with large bodies, with which I could exactly follow the motions of the *electric fluid*. But I could not suppose it easy for every experimental philosopher to procure this apparatus, which I had partly constructed myself; therefore I attempted to produce a small apparatus, containing in itself all the parts of the large one, which might easily be obtained by every experimental philosopher; and having succeeded, I thus introduced, in the same paper, this new plan of experiments on *friction*. " Mr. " Cavallo has given a table containing the results of his experi- " ments of this kind, wherein is found, that certain bodies " become either *negative* or *positive*, according to those by " which they are *rubbed*. *However, there remained to be known* " *what effect was produced on each of the bodies which exercised* " *that friction*. This has been one of the objects of my expe- " riments; for which purpose I kept *insulated* both bodies, " exercising *friction* on each other, applying *electrometers* to " both."

The apparatus.

9. Then follows, in the same paper, the description of the

apparatus with which these experiments were made: its figure, which is at the head of the paper, is half the size of the apparatus itself; and it may be seen, in that figure, that it is, in fact, a very small *electric machine*, with a *revolving part* and a *rubber*: but it is so constructed, that both these parts may be easily changed, for producing *friction* between different bodies, the effects of which are always shown by the *gold leaf electrometers*. I do not think it necessary to compare directly every part of these experiments with Dr. Maycock's theory; he is so intelligent, that, had he read my paper, he would have found himself those relating to the objects on which we dissent; therefore, I shall only indicate briefly some of these points.

Dr. Maycock's fourth proposition refuted by experiment.

10. The fourth proposition of Dr. Maycock's theory, above quoted, is the following: "The *friction* of two bodies, *similar* "in every respect to one another, produces no *excitement*." This is the immediate consequence of his theory, but is contrary to mine: here, therefore, is afforded a criterion between them; and he might have found the decision in my paper. There, after having explained my theory,—that, in the *friction* between two bodies, which operation agitates the *electric fluid* on their surface, the body which is the most disposed to seize upon that fluid, and to transmit it to its remote parts, becomes *positive*, and the other *negative*,—I added: "This holds, not "only between bodies of *different natures*, but even between "the *same kinds* of bodies, if one be made to pass in *length* "over *one part* only of the other. This effect cannot be ob"served with perfect *conductors*, as on them the *equilibrium* of "the *electric fluid* is instantly restored; but there is a known "experiment with two pieces of the same *silk riband*, in "which, by making one piece pass rapidly in *length* on *one* "*part* only of the other, the former becomes *positive*, by car"rying off some *electric fluid* from the latter, which thus is "rendered *negative*, by losing that fluid."

Experiment with silk ribands.

11. These experiments I have repeated many times; by using pieces of wide and strong *silk riband* about a yard long, at the extremities of which were fixed proper pieces of wood, to keep them stretched; one being held very steady, while somebody made the other pass rapidly on one part of the former: then applying each of them instantly to the top of a

gold leaf electrometer, the *riband* which has moved is found *positive*, and the other *negative*. I must observe, that this experiment cannot succeed, but when the air is very *dry*, commonly in winter, at the time that a divergence produced in the *gold leaves* by any cause is long preserved; else the effects produced on the ribands is soon dissipated.

with glass,

12. I have produced the same effect by the *friction* between other bodies absolutely *similar to one another*, namely, *glass* and *glass*; as may be seen in Exp. 3 of the same paper. The *revolving body* was a *glass* cylinder, and the *rubber* a piece of the same *glass*. Now, the *revolving glass*, as the *riband* which passed in *length* over the other, carried off some *electric fluid* from the immovable *rubber*, and immediately transmitted it to the prime conductor of the small machine; so that, at every revolution, the *gold-leaves* connected with it increased in *divergence*, and at last *diverged* much as *positive*.

and with dissimilar substances.

13. All the experiments related in that paper demonstrate the same theory concerning the effects of *friction*; but I shall only indicate them shortly, as the details may be seen in the paper itself. In Exper. 1, a *brass* rubber acting on a *glass* revolving cylinder, the *brass* became *negative*, and the *glass* was made *positive*. This is the same effect produced by a *metallic amalgama* laid on the rubber of the electric machine. In Exper. 4, a *sealing wax* rubber applied on the same revolving *glass* cylinder, the *sealing wax* becomes *negative*, and the *glass* is *positive*. The latter, as being a better *conductor*, carries off a greater part of the *agitated electric fluid*. In Exper. 5 is seen a very singular case. Having used for *rubber* a piece of *India-rubber*, on the same revolving *glass* cylinder, according to the degree of *pressure*, sometimes the *glass* became *positive*, and the *rubber* then was *negative*; at other times the former was *negative*, and the latter *positive*. This case shows, that, between the *same bodies*, when they have a disposition to *adhere* to each other, *friction* may have inverted *electrical* effects, according to the degree, or parts, that the adhesion takes place.

A metal, when insulated, rendered either positive or negative, according to circumstances.

14. I come now to very remarkable changes in the *electrical* effects of *friction*, according to other circumstances. It has been seen above, in Exper. 1, that a *brass* rubber, applied to the revolving *glass* cylinder, became *negative*, and the *glass*

was made *positive.* But in Exper. 6, the same *brass* rubber being applied to a revolving cylinder of *sealing wax,* the latter was made *negative,* and the *brass* became *positive.* Thus, therefore, *brass,* though the best *conductor* as a *metal,* when it is *insulated,* and thus retains the effect produced on it by *friction,* shows, that it is rendered either *positive* or negative, according to the body which exercises friction upon it.

Sealing wax rendered positive by friction with certain bodies.

15. With respect to *sealing wax,* which is our common test to discover whether our *electroscopes* indicate the *positive* or *negative* state by their *divergence*; because *sealing wax,* when rubbed with the hand, or some cloth, becomes *negative*; exper. 7 proves, that *sealing wax* itself is made *positive* by *friction* with certain bodies. In this experiment, the same revolving cylinder of *sealing wax,* which before was become *negative* by a *brass* rubber, was made strongly *positive* by the *India-rubber.*

Other experiments, showing different effects on the same bodies.

16. Exper. 8 is farther illustrative of these differences of *electrical effects* produced by *friction* on the *same* bodies, according to those which exercise *friction* on them. The object of that experiment is one of the *India-beads,* the size and colour of a cherry, used by Indian women in necklaces or other ornaments, which consist of an inspissated vegetable oil. One of these *beads* I made to revolve by a glass axis, and applied to it successively a *brass* rubber, and a *sealing wax* one: the *brass* rubber rendered it *negative,* and became itself *positive*; but the *sealing wax* rubber made the same bead *positive,* becoming itself *negative.*

Deductions from these experiments.

16. All these experiments prove, first, that the distinction between *electric* and *anelectric* bodies was illusory; that none, in their natural state, are either *positive* or *negative.* With respect to *friction,* these experiments demonstrate, that this operation has no other effect than that of disturbing the *equilibrium* of the *electric fluid* on their surface, one of which, according to circumstances, retains *more* and the other *less* of that *fluid.*

17. If Dr. Maycock happens to see this abstract of the experiments contained in my former papers in your Journal, I think he may find, that every thing belonging to *electrical phænomena* is much clearer than he had imagined: he, however, encouraged natural philosophers to collect all the *known facts*

under some *theory*, as tending *to advance our intellectual improvement*; and he will now judge whether I have accomplished this purpose.

Supposed obscurity with respect to the action of the galvanic trough.

18. The last part of his paper will lead us to another field, where he finds *much obscurity*, but on which I think light will appear. This part relates to what he calls the *galvanic battery*, saying: "that all the opinions, which have been proposed to "account for it, are unavoidably *hypothetical*, and indeed very "*unsatisfactory*; and that, therefore, every *fact*, which relates "to it, deserves *attention*, although its application may not be "clearly perceived." This gives me hope that he will consider what I shall here explain; expressing, however, again my regret, that he has not known my paper in your Journal on the *galvanic pile*, an apparatus in which the causes and effects may be easily followed; but I hope to make them clear, even in the apparatus of *troughs*, the only one Dr. Maycock seems to have used. I therefore shall copy first what he says of his experiments.

Dr. Maycock's experiments with it.

"I filled one of the new *porcelain troughs* with an *acid fluid*, "so that the *metallic plates*, and their connecting *arcs*, were "completely covered. In this state, a *trough* of 10 *pairs of* "*plates*, 3 inches square, decomposed water very rapidly. "Anxious to know how far the division of the *trough* into "*cells* is requisite, I placed the *metals*, connected by the *bar*, "in a *trough* without *partitions*, and filled it with the same "kind of *acid*,—but no action ensued. The action which "took place in the first experiment appears *inconsistent* with "all our *theories*; and it seems not a little curious, since a "*communication* between the *cells* is not an impediment to "action, that no action was evinced in the second experiment. "It would afford me much pleasure, should these observations "call the attention of your readers to the theory of *electrical* "*excitement*." It has certainly been the case with me, and I shall now explain how I find his experiments *consistent* with each other, and also with my theory.

Attempt to explain the apparent inconsistency in them.

19. In the first of these experiments, the *trough* with *partitions* produced a series of *ten* distinct *pairs* of the *two metals*, which, being formed of plates 3 *inches square*, were sufficient to produce the effect described; as the *liquid* was a *conductor*, which transmitted undisturbed the effect of each *pair* to the

next on both sides; as does the *wet cloth* in the *galvanic pile.* But when the *plates* were entirely *immersed* up to the *bars* in the *liquid,* the latter being a *conductor* which embraced the whole, every difference between the *metals* in each intermediary *pair* was destroyed, and the effect was reduced to that of *one* single *pair.*

Apparatus to imitate the phænomena of the electric eel.

20. This will be shown by an analogous experiment, which, for another purpose, I made some years ago at Berlin, related in p. 253 of the 2d vol. of a work under the title of *Traité élémentaire sur le Fluide électro-galvanique,* published at Paris, in 1804. I had then in view the phænomenon of the *electric eel;* that fish which produces the *shock* while in *water.* I tried to imitate that *eel* by a *galvanic pile,* composed of 30 groups of *zinc* and *silver,* separated by pieces of cloth imbued with salt water. These groups were held together by 3 glass rods, so kept together as to leave no projection outwards, and resembling so far an *electric eel.* With this pile I made the following experiments:—1. It being held upright, I received a strong *shock* from it: having applied to it the usual *glass tube with water,* the *gasses* were produced in that tube. 2. I laid the pile on my table; it continued to produce the *shock.* 3. I laid it in a narrow *wooden trough,* with a little *water* at the bottom; the *shock* was less. 4. I poured successively more *water* into the *trough:* in proportion as the *water* rose round the *pile,* the *shock* was less; and at last, when the *water* covered it entirely, not only there was no more *shock,* but, having applied between its extremities a *glass tube with water,* no *gas* was produced. The *electrical eel,* therefore, has no perceptible analogy with the *galvanic pile,* though the effects are similar.

Experiments with it.

This, I think, will show Dr. Maycock the manner in which his two experiments are *reconciled* with each other, and are *consistent* with my *theory.* It will also give me much pleasure, sir, if Dr. Maycock, finding any objections to my explanation, will transmit them to me through your valuable Journal; for I have a great regard for him, though not personally acquainted with him.

I have the honour to be,
Sir,
Your obedient, humble Servant,
J. A. DE LUC.

Windsor, October the 5th, 1812.

VII.

METEOROLOGICAL JOURNAL,

1812.	Wind.	Barometer. Max.	Barometer. Min.	Barometer. Med.	Thermometer. Max.	Thermometer. Min.	Thermometer. Med.	Evap.	Rain.	
8th Mo.										
Aug. 29	N W	29·99	29·98	29·985	54	52	53·0	—	·16	☽
30	N E	30·04	29·99	30·015	61	53	57·0	—	·14	
31	N	30·15	30·04	30·095	60	48	54·0	—		
9th Mo.										
Sep. 1	N E	30·18	30·15	30·165	59	48	53·5	—		
2	S E	30·15	30·10	30·125	62	54	58·0	—		
3	S W	30·10	29·95	30·025	62	53	57·5	—		
4	S E	29·95	29·89	29·920	63	45	54·0	—		
5	N E	29·96	29·89	29·925	65	46	55·5	·28		●
6	N E	30·07	29·96	30·015	67	40	53·5	—		
7	S E	30·09	30·07	30·080	67	41	54·0	—		
8	E	30·07	29·97	30·020	69	50	59·5	—		
9	S E	29·99	29·97	29·980	68	47	57·5	·41		
10	S	30·17	29·95	30·060	67	47	57·0	—		
11	N W	30·28	30·17	30·225	68	42	55·0	—		
12	W	30·26	30·22	30·240	69	44	56·5	—		
13	Var.	30·18	30·14	30·160	71	42	56·5	—		☾
14	Var.	30·18	30·07	30·125	69	40	54·5	·53		
15	N W	30·07	29·94	30·005	68	41	54·5	—		
16	S W	29·94	29·82	29·880	72	47	59·5	—		
17	Var.	29·96	29·82	29·890	62	41	51·5	—	·15	
18	N W	30·20	29·96	30·080	60	37	48·5	—		
19	S W	30·14	30·12	30·130	60	46	53·0	·48		
20	S W	30·12	30·03	30·075	69	45	57·0	—		○
21	W	30·03	29·97	30·000	73	47	60·0	—		
22	S W	29·95	29·91	29·930	68	50	59·0	—	·34	
23	S W	29·95	29·93	29·940	57	44	50·5	·57		
24	N W	30·06	29·95	30·005	56	34	45·0	—		
25	Var.	30·04	29·99	30·015	60	40	50·0	—		
26	W	30·07	30·04	30·055	63	53	58·0	·23		
		30·28	29·82	30·040	73	34	54·93	2·50	·79	

The observations in each line of the table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

REMARKS.

Eighth Month. 30. Very showery. Between 4 and 5 p. m. a sudden *tornado* (as it seems by the description given) crossed the village in a direction from N. E to S. W. which left behind it considerable traces of its violence : a large quantity of wheat in sheaves was carried over a hedge into a neighbouring field : a fence was levelled, and about seventy *oak hurdles* torn out of the ground, some of which were seen tumbling over in the air, and fell at two hundred yards distance.

Ninth Month. 12. Misty morning : much dew. 13, 14. The same in the evening, a dense *stratus* reflecting on its surface with much brilliancy the orange colour of the western sky. 15. Hoar frost in the pastures : a *stratus* at night as before, the wind coming about to the eastward soon after it was formed. 16. Cirrus with cirro-stratus and cumulus. 17. Rain most of the afternoon ; a rich crimson tinge on the lower surface of the clouds at sunset. 18. At sunset the sky was extensively coloured with orange, surmounted by a distinct blush of red : the colour was reflected in the E. horizon. 19. a. m. much hoar frost. 22. a. m. clear at first, but soon overcast, with rain· 25. Hoar frost. 26. Cirro-stratus.

RESULTS.

Easterly winds prevailed the fore part, and westerly the latter part, of this period.

Barometer : highest observation 30·28 inches ; lowest 29·82 inches,
Mean of the period 30·040 inches.

Thermometer : highest observation 73° ; lowest 34°.
Mean of the period 54·93°.

Evaporation 2·50 inches. Rain 0·79 inches.

Plaistow. L. HOWARD.
Tenth Month, 8, 1812.

VIII.

On the primitive Crystals of Carbonate of Lime, Bitter-Spar, and Iron Spar. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S*[*].

Angles of crystals corrected by the author's goniometer.

WHEN I formerly described to the Society a goniometer[†] upon a new construction for measuring the angles of crystals, I expressed an expectation, that we should thereby be enabled to correct former observations made by means of less accurate instruments. I took occasion to mention one instance of inaccurate measurement in the primitive angle of the common carbonate of lime; and I have had the satisfaction to find the necessity of a correction, in that instance, confirmed by Mons. Malus, and admitted by the Abbé Haüy, in a work[‡] published nearly at the same time.

Haüy's accuracy in general surprising.

It is by no means my design to detract, in any degree, from the merit of that justly celebrated crystallographer, to the surprising accuracy of whose measurements I could, in various instances, bear testimony. I hope, on the contrary, that in bringing forward two more observations similar to the preceding, and intimately connected with it, I shall offer what will not only appear interesting to crystallographers in general, but will be peculiarly gratifying to the Abbé Haüy.

The same primitive form assigned by him to three substances.

In his Traité de Minéralogie, and again more recently in his Tableau Comparatif, the same primitive form is assigned to three substances very different in their composition to carbonate of lime, to magnesian carbonate of lime (or bitter-spar) and to carbonate of iron.

Identity of composition should accompany identity of figure.

It has been objected to Mons. Haüy, that according to his method identity of form should be accompanied by identity of composition, unless the form were one of the common regular solids. For though in this case any geometrician would readily admit it to be very probable, that many different substances might occur in assuming the same form of cube, of octohedron, or of dodecahedron, &c.; there does not appear a corresponding probability, that any two dissimilar substances would assume the same form of a particular rhomboid of 105° and a few minutes, to which no such geometric regularity, or peculiar simplicity, can be ascribed.

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[*] Philos. Trans. for 1812, p. 159.

[†] Philos. Trans. 1809, p. 253; or Journal, vol. XXV, p. 192.

[‡] Tableau Comparatif des Résultats de la Crystallographie et de l'Analyse Chimique.

The forms of the three carbonates similar, but not identical.

But though so accurate a correspondence, as has been hitherto supposed to exist in the measures of the three carbonates above-mentioned, might be justly considered as highly improbable, no degree of improbability whatever attaches to the supposition, that their angles approach each other by some difference, so small as hitherto to have escaped detection. And this, in fact, I find to be the case.

Least measure of an angle usually employed by the author.

Since the angles observable in *fractures* of crystalline substances are subject to vary a little at different surfaces, and even in different parts of the same surface (as is evident from the confused image seen by reflection from them) I shall not at present undertake to determine the angles of these bodies to less than five minutes of a degree. This, indeed, is the smallest division of the goniometer that I usually employ, as I purposely decline giving so much time to these inquiries, as would be requisite for attempting to arrive at greater precision.

Primitive angle of carbonate of lime,

The most accurate determination of the angle of carbonate of lime is probably that of Mons. Malus, who measured it by means of a repeating circle, and found it to be 105° 5′. And this, indeed, is the result to which I formerly came by a different method[*]. If it differ in any respect from this quantity, I am inclined to think that it will more likely be found to be deficient by a few minutes, than to exceed the measure here assigned; and accordingly to differ still more widely from those angles which I am about to mention.

of bitter-spar,

In the magnesian carbonate of lime, or bitter-spar, the primitive form is well known to be a regular rhomboid, as well as that of carbonate of lime, and so nearly resembling it, as to have been hitherto supposed the same I find, however, a difference of 1° 10′ in the measures of these crystals; for that of the magnesian carbonate is full 106¼°, as I have observed with uniformity, in at least five different specimens of this substance, obtained from situations very distant from each other.

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[*] Phil. Trans. 1802, p. 385; or Journal, vol. IV, p. 150.

and of iron-spar.

The primitive angle of iron-spar is still more remote from that of the carbonate of lime, which it exceeds by nearly two degrees. I have examined various specimens of this substance, some pure white, others brown, some transparent, others opake. That which gives the most distinct image by reflection is of a brownish hue, with the semitransparency of horn. It was obtained from a tin mine, called Maudlin Mine, near Lostwithiel, in Cornwall. By repeated measurement of small fragments of this specimen, the angle appears to be so nearly 107°, that I cannot form any judgment whether in perfect crystals it will prove to be greater or less than that angle.

In this instance the carbonate of iron is nearly pure, and so perfectly free from carbonate of lime, as to render it highly probable, that in other specimens, having the same angle, but containing also carbonate of lime, or other ingredients intermixed, the form is really dependent on the carbonate of iron alone.

Pearl spar. Mixtures probably most liable to curved surfaces.

It appears, however, not unlikely, that when substances, which agree so nearly in their primitive angle, are intermixed in certain proportions, they may each exert their power; and may occasion that confused appearance of crystallization with curved surfaces, known by the name of pearl-spar. I cannot say that I have made any accurate comparative analyses, which may be adduced in support of the hypothesis, that mixtures are more subject to curvature than pure chemical compounds; but it is very evident, from the numerous analyses that have been made of iron-spar by other chemists, how extremely variable they are in their composition, and consequently how probable it is, that the greater part of them are to be regarded as mixtures; although it be also possible, that there may exist a triple carbonate of lime and iron as a strict chemical compound.

Perhaps some carbonate, modified by manganese, exists.

It seems not unlikely, that there may hereafter be found some carbonate allied to the preceding, which may owe its form to the presence of manganese; but notwithstanding the liberality which happily prevails in general among those who have it in their power to assist in such inquiries, I have not had the good fortune to meet with any such compound; and I am unwilling, merely in the hope of making such an addition,

tion, any longer to defer communicating an observation, which I hope will be of real utility in the discrimination of bodies that differ so essentially in their composition.

IX.

Account of an Economical Lamp for producing Heat, with a considerable saving of Oil. In a Letter from a Correspondent, L. O. C.

To Mr. Nicholson.

SIR,

IF you should not think the following experiments too trifling for insertion in your excellent Journal, it is probable, that such of your readers as are in the habit of using an Argand lamp in their chemical researches may feel interested in them. They certainly have some claim to attention in an economical point of view: and this, I think, no one will deny, when he is informed, that he may procure a lamp at one tenth of the price of the cheapest Argand lamp; which will produce an equal, if not a greater degree of heat; and effect a saving of at least one third of the quantity of oil consumed.

Introduction. Economical lamp costs one tenth of an Argand lamp, and saves one third of the oil.

Experiment 1. a. An Argand lamp, which is supplied by a fountain, so that the oil can never become very hot, was made to burn as bright and strong as possible, without smoke; and was found to consume 447 grs. of oil, in an hour. Two thousand grains of rain water were exposed to the heat of this lamp in a glass matrass; they boiled in seven minutes.

Exp. 1. Argand's lamp consumed 447 grs. of oil per hour, and boiled 2000 grs. of water in 7 m.

b. As I suspected that the lamp did not produce its greatest effect, on account of the wick having been in the oil for a length of time, I caused a new one to be put to it, and every precaution was taken to make it burn as powerfully as possible. It now consumed 500 grs. of oil in the hour, and made 2000 grs. of water to boil in 6$\frac{1}{3}$ m.

Same lamp, newly trimmed and filled, consumed 500 grs. per hour, and boiled the water in 6$\frac{1}{3}$ min.

Exp. 2. I caused a small tin lamp to be made, with four burners; and having a tube in the centre, to convey air to them; each of the burners containing eight threads of cotton, of the diameter of $\frac{1}{40}$ of an inch. This lamp, when burning as strongly as possible without smoke, consumed in one hour 200 grs. of oil, and made 2000 grs. of water boil in 10 m. (See plate 5, fig. 3 and 4; and the description at the end of this letter.)

Exp. 2. A lamp with 4 separate small wicks, &c. consumed only 200 grs. of oil per hour, and boiled the water in ten min.

It saved more than one third of the oil.

We may here observe, that the Argand lamp expends, in raising 2000 grs. of water to the boiling point, 52·1 grs. of oil; and that the lamp just described requires only 33·3 grs. to produce the same effect, which is a saving of 18·8 grs. or more than one third.

Exp. 3. Another lamp, but with the same cotton, in 8 separate wicks, consumed 300 grs. of oil per hour, and boiled the water in $6\frac{1}{2}$ min.

Exp. 3. Although the lamp described in the last experiment did not produce an effect nearly equal to an Argand lamp, in a given time; yet I could clearly perceive that the principle upon which it was constructed was good, and capable of being carried to a much greater extent. I therefore ordered another to be made, in every respect similar to the former; except in the number of burners, which were increased to eight, each of which contained four threads; so that the quantity of cotton in the wick of each lamp was exactly the same. This lamp consumed 300 grs of oil in an hour, and made 2000 grs. of water boil in $6\frac{1}{2}$ m. Its power, therefore, was equal to that of an Argand lamp; and if we take the quantity of oil consumed by an Argand lamp in heating to ebullition 2000 grs. of water in $6\frac{1}{2}$ m. at 45 grs. (which is evidently too small a quantity) and that consumed by this lamp, in producing the same effect, at 30 grs. the quantity required by the two lamps to produce a given effect, in a given time, will be in the ratio of 2 to 3.

Its effect was therefore equal to that of the Argand lamp; but one third of the oil was saved.

Exp. 4. smaller wicks consumed more oil, without increasing the effect.

Exp. 4. a. The same lamp was used in this experiment, as in the preceding; but as four threads appeared to fill the tubes too tight to admit the free ascent of the oil, only three were put into each of them; and a glass, $1\frac{1}{2}$ in. in height, and 2 in. in diameter, was placed over the flame, as in an Argand lamp. When thus adjusted it consumed 320 grs. of oil in an hour; and caused 2000 grs. of water to boil in $6\frac{1}{2}$ m.

When the chimney glass was shortened, the effect was increased;

b. As the effect appeared to be somewhat diminished by the elevation of the matrass to too great a distance from the flame of the lamp, this glass was removed, and another, only 1 in. in height, substituted in its stead. The flame did not appear to be as vehement now as before: it nevertheless produced a greater effect, which was owing, no doubt, to the bottom of the matrass being nearly in contact with it. Only 276 grs. of oil were consumed in the hour; and 2000 grs. of water boiled strongly in $6\frac{1}{4}$ m.

and the saving nearly one half.

This appears to be the preferable mode of using the lamp, as it thus produces a greater degree of heat than an Argand lamp, even under the most favourable circumstances; and consumes, in the same time, 224 grs. less of oil, which is a saving of about $\frac{9}{20}$, or nearly half. If we calculate the quantity required by each to produce the same effect, it will be still more in favour of mine.

Exp 5. The effect of Argand's lamp is less when the glass is taken away.

Exp. 5. a. As the advantages arising from bringing the substance to be heated, as near as possible to the flame of the lamp, appeared to be so considerable, it was natural to conclude, that similar effects would attend the shortening of the glass of the Argand lamp, or the removing it altogether. Accordingly, 2000 grs. of water were exposed to the heat of this lamp, when burning as clear as possible, without a glass. The water did not boil in less than 7¾ min. This, therefore, did not appear to be any improvement.

But a short glass of 1½ inch long, saves one fifth of the oil in the A. lamp.

b. I added to the Argand lamp a glass 2 in. in diameter, and 1½ in. in height, from the base of the flame. Its power was considerably increased, for it made 2000 grs. of water boil in 6½ m. and consumed only 400 grs. of oil per hour.

Those who do not think it worth their while to procure a lamp similar to that which I have recommended, will at least find it a considerable improvement, to reduce the glasses of their Argand lamps, to the dimensions here specified, which will cause a saving of one fifth of the quantity of oil at present consumed.

Exp. 6. The heat of the new lamp is steady, and is proportioned to the number of burners.

Exp. 6. One of my principal objects, in the construction of this lamp, was, that I might be able always to procure an equal degree of heat; and one that could be augmented or diminished uniformly. The following experiments were instituted with a view to determine, whether it possessed these properties or not. I have arranged them into a table;

	m.	m.
8	3½	
7	4	4
6	4¾ hardly	4⅔
5	6 nearly	5·6
4	6¾	7
3	9½	9⅓
2	15	14
1	30	28

the first column of which indicates the number of burners employed; the second, the time required to raise 1000 grs. of rain water to the boiling point; and the third, the time required by calculation to produce the same effect; taking the first experiment for the standard, viz. that with eight burners, the lamp will make 1000 grs. of water boil in 3½ m., which is very nearly half the time which it requires to make 2000 grs. boil.

The real times are copied exactly from the memoranda I made while performing the experiments; and I was much surprised to find how nearly they coincided with the times by calculation: especially since the watch used had no seconds hand. In the last experiment but one the water never boiled strongly; and in the last it only just simmered. I have added a sketch and description of the lamp, for the use of those who may wish to procure one like it. Its dimensions are exactly double those of the drawing; with the assistance of which any tinman will be able to make it; and I should recommend it to be made with a reservoir, to prevent the oil from being much heated, as I have found by experiment, that this circumstance considerably increases the quantity of oil consumed in a given time, without adding, in the least degree, to the heat of the lamp. The price of one, similar to the drawing, will not exceed 1s. 6d. exclusive of the glass, which is about one tenth of the price of the commonest Argand lamp that I have seen.

Hot oil is consumed faster without augmenting the effect. Hence a reservoir is advisable.

I am, Sir,

Your constant Reader,

L. O. C.

Description pl. V, fig. 3, 4. A the body of the lamp; B. B. the burners; C. tube for conveying air to the flame; D. D. D. small feet which raise the body of the lamp, and allow the free access of air to the tube C.; E. E. bended wires, which support the glass F.; G. small tube for filling or emptying the lamp*.

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* It would afford an acceptable result, if the writer would also measure the light afforded by the lamps under comparison, as well as the heat. As the method of shadows, though very simple and easy, is certainly not in general use, I would here repeat, that when the shadows of the same object, projected upon a wall or surface by two lights, are equally dark, the lights themselves are equally intense—that, if not, the darkest shadow will be projected by the interception of the brightest of the lights; and that, if this brightest light be then removed farther from the wall, till both shadows become equally dark, and the distances of the lights from the wall be in that situation measured, the intensity of each

X.

Description of a new Construction and Arrangement of the Keys of Musical Instruments, invented by JOHN TROTTER, *Esq.* (W. N.)

General account of keyed instruments.

The principal series, or natural notes expressed by simple characters, and afforded by white keys.

Notation by flats and sharps condemned.

IN the piano forte and other instruments, in which the notes of the musical or diatonic scale are afforded by wires, strings, pipes, or other parts of invariable figures, lengths, or other affections, and produced by the action of keys,-it is well known, that one principal series has its notes designated by the first seven letters of the alphabet, A, B, C, D, E, F, and G; and that the intervals of gravity or acuteness are all (nearly) equal to each other, except the interval between B and C, and that between E and F, which are (nearly) half the magnitude of the others; and that by interposing other notes at (nearly) equal distance between each of the greater intervals, a series of such smaller intervals (called the intervals of semitones) is constituted, and affords the number of twelve notes in all; out of which it is practicable, by commencing from any individual note, to take a regular diatonic series, having its notes and half notes disposed in a similar manner to those of the principal series before mentioned. The present occasion does not require any illustration of the nature of the musical scale, or the degrees of imperfection to which the secondary scales of fixed instruments are unavoidably subject, and the remedy usually afforded in part by temperament. Practical musicians are aware, that the white keys of a piano forte, and the lines and spaces of a music book, are appropriated to the notes of the principal series, called the natural scale; and that the auxiliary half notes are given by black keys on the instrument, and marked in the book by the same character as denotes the note next ascending or descending, but have the modifying character of flat or sharp prefixed in the cliff or elsewhere. That this mode of notation is unscientific and awkward; and that the correspondent structure of the keys is irregular and embarrassing, will scarcely be controverted by any one who shall maturely consider the subject. In the natural

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each will be in proportion to the square of its distance. For example; if two lights give shadows equally black or dark, when their distances from the wall or surface are respectively five and seven feet, the intensity, or quantity of light emitted from them, will be respectively as 25 (or 5×5) and 49 (or 7×7).—N.

series, every character simply indicates its note; but if we take the fundamental note one tone higher, viz. from C to D, then two sharps, namely *f* and *c*, will be marked in the cliff; and the same characters, in effect, will no longer indicate the same notes, or require the same keys to be touched. If we assume the fundamental two tones above C, four sharps will be marked in the cliff, &c. And it is accordingly found, that in transposing and performing on the different keys, the fingering is very different in each. Mr. Trotter has found a simple and effectual remedy for this last difficulty, by constructing the keys in such a manner that no preference as to white keys is given to any particular series; but every series is fingered precisely in the same manner. He places the white and black keys alternately in succession for every one of the twelve semitones; and the consequent rule is, that for the sharp series ascending from any note whatever, the key note itself is to be touched, and then the two keys next ascending of the same colour, and these are to be followed by the four keys next ascending of the opposite colour. Thus in fig. 5, pl. V, the key of the Note C being black is to be followed by the black keys of D and E, and afterwards by the four white keys of F, G, A, and B. And it is easy to show, that in the flat series a rule no less simple and universal prevails throughout, viz. that the key note, and the next above, and two next below it, are to be of the same colour, and the other three of the opposite colour.

Mr. Trotter places a black and a white key in regular succession for all the twelve semitones; which affords a simple and invariable rule for performing in every key of music.

Illustration by the engraving.

In the same figure, where W, W, &c. represent the white keys, and N, N, &c. the black keys as usual, the letters *n*, *n*, represent the same black keys continued, and a little depressed below the white ones. By this means the performer has his election to touch either N or n of any key at pleasure; and as the nature of the passage may require. This construction is shown in profile at fig. 6.

Performance on the instrument by a professional man.

I was present when a professional gentleman, who had practised on this improved instrument two days, performed several pieces of music upon it, which were of difficult and rapid execution. He spoke with much approbation of the facility and advantages it affords, and particularly remarked, that the improvement *n*, *n*, &c. allows certain passages to be performed in a superior manner, which, if fingered by means of the faces

N. N. &c. would be much less manageable. Not being myself a performer on any keyed instrument, I can only express my approbation in general terms. Letters patent have been granted for the invention.

XI.

A new Compensation Pendulum, without Joints or Surfaces bearing against, or moving upon each other. In a Letter from a Correspondent. (R. B.)

To Mr. Nicholson.

SIR,

SINCE the invention of pendulums of compensation for changes of temperature, there have been a number of excellent contrivances, by means of the contrary expansions of metallic bodies, for keeping the centres of oscillation and suspension at an invariable distance from each other. In the quicksilver pendulums, or any of the contrivances which require the use of a fluid, it may reasonably be supposed, that the changes, which are to compensate each other, do take place at the same time as the temperature becomes altered; and that the quantity and effect of the compensation continue the same for any length of time, during the existence of the machinery. But in pendulums composed altogether of solid materials, both these results have been called in question. In the gridiron pendulum, it has been insisted, that the adjustment of the expansions is by the construction of holes and pins, not capable of extreme exactness; and that the variation, to which the pressure of contact may be liable, and the stickage of the bars, (which rub against the frame) may be more than sufficient to counteract and give uncertainty to all small variations, required to be compensated. And in all constructions of bars of steel and brass, soldered or connected lengthwise, it has been doubted—not only whether the wire-drawing and upsetting of the elastic metals themselves, under a state of such severe force, may not allow for all small changes, without perceptible flexure,—but likewise whether the flexure, which in larger changes is perceived, may not become altered, after a course of time, and long exposure to the effects of heat and cold. Perhaps, Mr. Nicholson, you, or your readers, may be inclined to

Pendulums of compensation.

Mercurial.

Gridiron pendulum objected to.

Expansion bars supposed to alter by the forcible action of their parts.

Disquisitions upon minute changes in bodies useful.

to think these speculations the mere creatures of theory; and may, from the actual performance of many surprising time-measurers, be disposed to conclude, that they may be practically of no consequence. But I would urge, Sir, that it is by this kind of minute and scrupulous investigation, that discoveries come to be made; and that, while the best makers hold different opinions, and with all their skill (which I truly honour) cannot foretel, beyond a certain point, whether the work they are upon will answer their expectation or not,—we may actually expect still to make discoveries in this useful and curious pursuit. And while we make our experiments and operations under the guidance of some probable hypothesis, or doubt, or indication, the objects we produce may, in many instances, be found to possess unforeseen advantages, or serve to establish new truths, perhaps not suspected by the operator himself.

New pendulum.

I have been desirous of constructing a pendulum, composed of expansion bars, not subject to the violence of soldered or adherent faces pulling against each other, or the uncertainty of contacts, the rub of surfaces, the stickage of joints, or the bearing parts of levers. The following has been going for several years.

Construction. Two pair of strait bars, each of brass without, and steel within, are rivetted together, and each pair to the other at one end only. Flexure allowed at certain thin parts, &c.

In the annexed drawing, (Fig. 2, plate v.) f g and i h represent bars of brass, respectively united by rivetting and soldering with two bars of steel at the ends where they touch, but their faces do not touch except near the ends, as shown in the drawing.

At the extremity g l n i of the system, all the four bars are connected, as is also shown in the cross section p; but at the other extremity the bars f k are connected, and so likewise are the bars m h; but there is no union between k and m. At o, o, the bars are filed away or notched, so as to leave each of them thin on the side farthest from the middle line of the system. a d shows part of the pendulum rod, of which a is the point of suspension, connected by a spring with b, a clamp adjustable by sliding along the face of f g, and fixable in any required position by the screw c. And d is part of the lower rod, broken off to save room in the plate instead of being continued down to the ball.

Particular description of the

The effect of this combination may be thus explained: When by an increase of temperature the brass bars f g and i h become longer effect upon change of temperature.

longer than their correspondent steel bars $k\,l$ and $m\,n$, the whole of the flexure will take place without any strain or tension of any practical importance, in the thin parts at o, o, and the faces $k\,m$ will be brought nearer together, and so likewise will a and d; by which means the pendulum rod will be shortened, and this shortening may, by the due adjustment of the clamps, be made precisely to compensate for the lengthening of the rod, caused by its direct expansion by the same increase of temperature.

Hardy's balance.

I believe the happy expedient of confining the flexure to a thin part of the bar was first used by Mr. Hardy, in his expansion balance.

Principles of computation, for the quantity of effect.

In order to show the quantity of effect, let fg, kl (Fig. 2*) represent one pair of the bars, of which og, cl are the thin parts, and co the depth of the notch; and suppose oe to be the effect of the expansion by one degree of heat in the thin brass part beyond that of the steel bar; which will be 0·00000331 in unity.

Or, go being equal to co, the quantity eo will be $=$ 0·00000331 × depth of notch.

But the whole excess of expansion will be greater in proportion as the whole bar is longer than co, or the depth of notch, that is, 0·00000331 × length of bar = whole excess of expansion.

And as this excess will cause an angular deviation in the line co to ce and beyond, and will also cause a similar deviation in gf, the linear deviation of the extremity f will be greater than that of o, in proportion as the whole bar is longer than co, or the depth of notch; that is, depth of notch : length of bar :: excess of expansion = 0·00000331 × length of bar : linear

$$\text{deviation of } f = \frac{\overline{\text{length of bar}}^2 \times 0{\cdot}00000331.}{\text{depth of notch.}}$$

In order to adjust the compensation, the effective length of the bar is variable by means of the clamps, and the deviation of the parts of the pendulum rod, above and below the set of bars from a precise right line, would not exceed ten minutes, if the parts were inflexible, and the bars very short; but the spring of suspension is practically sufficient to preserve the right line.

XII.

Abstract of an Essay on the Construction and Effects of the Pneumatic Tinderbox, by Le BOUVIER DESMORTIERS[*].

Production of fire by compression.

THE inflammation of spunk in the pneumatic tinderbox, by the compression of air alone, is a phenomenon, with which chance, the father of discovery, has lately enriched natural philosophy. Many have reasoned on its cause; which some consider to be caloric, others electricity; but no one, that I know of, has attempted to support his opinion by experiments[†]. Without bias for any hypothesis, I have made some researches on the construction and effects of the pneumatic tinderbox, the results of which shall be the subject of the present paper. In the first part, I shall consider what relates to the structure of the instrument; in the second, I shall give an account of the experiments, that tend to the discovery of the cause of its effects.

The piston generally too long.

I. The first construction of these tinderboxes was a little faulty in the piston being commonly eighteen or twenty lines long. This was said to be necessary, that the air might not escape, when the piston was in action; for, if there were any point not accurately fitted to the inside of the tube, the air escapes, and the spunk does not kindle.

Half an inch, with a tube of six inches, sufficient.

The goodness of the instrument does not depend on the length of the piston, but on the accuracy with which it fills the bore of the tube; with a tube well bored and a piston of six lines, the air will no more pass than with a piston of twenty. Accordingly, for a tube of six inches I have reduced the piston to six lines, which adds an inch to the column of air, and diminishes the friction two thirds, so that the effect of the tinderbox is more certain, and it is more easily used. With a little dexterity you may kindle the spunk by holding the tube in one hand and pushing the piston with the other, without being obliged to rest it on a table, or any other solid body. Mr. Dumotiez, a skilful maker of philosophical instruments, is so fully convinced of the advantage of short pistons, that he now makes them of these dimensions.

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[*] Journal de Physique, Vol. LXVII, p. 125.

[†] See Journal, Vol. XX, p. 278; and Vol. XXI, p. 234.

They

Short pistons of advantage in other machines.

They should be employed also in the syringes of air guns*, of fountains acting by compressed air, of the apparatus for artificial mineral waters, of fire-engines, which are worked with so much labour, and even of air-pumps. As the shortening the piston is an advantage to the pump, we obtain a greater effect with less labour, and in a shorter time, than with long pistons.

The chamber must be air-tight, but not essential for the piston to be so.

It is essential too, that the instrument does not leak at the part where the spunk is placed, because there the transient action of inflammation takes place, and a slight emission of air would prevent the effect. But this effect is produced, though the piston does suffer the air in the tube to pass it. To satisfy myself of this, I made the following experiment, at which they who have seen it were greatly surprised.

Four grooves made in the piston did not prevent the effect,

In the length of the piston I made a groove a quarter of a line broad. The spunk took fire as before. Three other grooves were added successively opposite one another, so as to divide the piston into four equal parts; and still the spunk took fire†. When the grooved piston is moved backwards and forwards in the tube, the air may be heard entering or issuing out; and the friction is so slight, that the effect of the instrument is easily obtained by pushing it with the hand. This kind of piston would be preferable to those that fit accurately, if a solid substance were employed, hard enough to resist the continual friction of the air passing through the grooves, if I may be allowed the expression. The grooves in leather pistons soon alter their shape, and spread so as to allow the air to pass in too large quantity.

but one of larger dimensions did.

The piston with four grooves acting very well, I made one with a single groove, of dimensions equal to the other four, and what I foresaw actually took place: there was no inflammation. The following are the reasons of this difference.

Why the small grooves do not prevent the action,

The extremity of the grooved piston exhibits the area of a circle, the peirphery of which touches the interior edge of the grooves. The column of air contained in the tube rests almost wholly on this base. There are only the parts corresponding to the grooves, that are continued through the length of the piston, and communicate with the external air. When the piston is pushed with sufficient velocity to kindle the spunk, the parts of the column corresponding to the grooves rush into them with equal velocity; but the friction they experience in passing through such narrow tubes occasions a resistance to their passage, a kind of choaking, that suffers only a part to escape, while the column resting on the area of the piston is pushed entirely toward the extremity of the tube, where the spunk to be kindled lies.

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* In the air-guns of Germany, which are the best we know, the piston of the syringe is extremely short.

† I tried this experiment with Mr. Bancks, at his house in the Strand, and we found it succeed completely with a common condensing syringe of his making.—C.

and the large one does.

In the piston with a single broad groove, the area of the circle, on which the column of air rests, is much smaller, consequently the column itself is less. The resistance the air experiences in passing through the groove is next to nothing; for we hear no noise on moving the piston backward and forward; and as air expands in all directions, when the piston is moved, the column resting on the area of the circle, resting at the same time laterally on that which answers to the groove, it recedes from all the points of contact, and flows entirely through the channel it finds open. It is so true, that it wholly flows out, that the piston, when it touches the extremity of the tube, remains there; while with other pistons a sufficient quantity of air is retained to occasion a spring and repel them.

Choice of the touchwood.

I think it proper to say a word or two on the quality of the spunk. The driest, softest, and least impregnated with nitre, should be chosen. In that of the best quality a piece will not always be found equally good throughout. Some contains a great deal of nitre, and is kindled with more difficulty*. This may be known by the cool taste it leaves on the tongue; or by kindling it: for when it has taken fire the nitre melts, and sometimes throws out sparks, that may be dangerous when they spirt out of the instrument, particularly if made with a cock. As it is usual to blow on the spunk, to try whether it be kindled, a spark may be thrown from it into the eye. This painful accident once happened to me.

Danger of blowing on it.

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* Spunk is prepared from agaric, which is first boiled in water; beaten well when dry; steeped in a strong solution of saltpetre; and lastly dried in an oven. If the solution of nitre be too strong, the agaric is loaded with this salt, which retards its inflammation.

They

Electricity supposed to be the cause of the fire.

They who imagine, that electricity kindles the spunk, consider these sparks as an incontrovertible proof of their opinion. I think they are mistaken in this case; yet I must not conceal a fact communicated to me by Mr. Veau-Delaunay, which seems to confirm this opinion, of which he is a partisan. Out of twelve times, when he operated with the instrument without any spunk in it, he saw sparks emitted three times. There are strong reasons, however, for suspecting, that electricity is not the cause of the inflammation here. These I shall give in the second part of this paper, concluding the present with an important observation on the construction of pistons.

On the construction of pistons.

Attempt to use elastic gum for them.

If we could find an elastic substance sufficiently compact to be turned in a lathe, we should have perfect pistons, that would spring and adapt themselves to the inequalities of the tube, without suffering a bubble of air to escape. I have made some with caoutchouc, softened before the fire, in order to give it a degree of elasticity more obedient to the inequalities of the tube. But on attempting to turn it in a lathe, it bent under the tool. Even the edge of a razor would not take hold of it; so that the piston remained uneven and almost ragged, and yielded like soft wax under the fingers. In this imperfect state it so far prevents the air from escaping, that a column of three inches is sufficient to kindle the spunk; but after a few strokes of the piston the heat dilates it to such a degree, that it cannot be moved without considerable force. If a drop of oil be put on it, it moves easily; but this soon spoils the instrument; for the oil dissolves the caoutchouc, and forms a varnish, which, as the piston grows hot, makes it adhere still more strongly to the sides of the tube.

This might be covered with leather.

Might not these inconveniences be avoided, by arming the piston rod with caoutchouc, and covering this with leather? If this process succeeded, it might be applied with advantage to all sorts of pumps.

What is the cause of the inflammation?

II. To attain, if possible, a knowledge of the principle of inflammation in the pneumatic tinderbox, four things are to be considered—the materials of the tube, the matter contained in the tube, the materials of the piston, and the friction. Among the materials of the piston I include the grease, with which it is coated, to make it move more easily, and render it fitter to intercept the passage of the air.

In

Is it electricity?

In examining the question whether the spunk be kindled by electricity, I consider

Arguments against this.

1st, That no part of the instrument is insulated; and that insulation is a necessary condition for producing sensible electricity with any of the machines we know. I say machines that we know, because the animal electricity, that manifests itself without insulation, is an exception to our mechanical means, and cannot here be taken into consideration.

2dly, The friction of the piston, which is a greasy body, against a metallic substance, is not calculated to produce electricity.

3dly, Experience demonstrates, that, unless during storms, the atmosphere seldom exhibits any signs of electricity at the height in which we breathe it; and that we must search for them with instruments in a more elevated region, or when electric clouds are passing over our heads. How then shall we estimate the infinitely small quantity of electric matter in a cubic inch of air, or even less, which the instrument contains.

Fourthly, It is not without great difficulty, that we can kindle spunk with strong electric sparks. I have discharged a large jar on spunk strewed with powdered resin, and it has remained unkindled, though the resin caught fire, and burned entirely away.

In a metallic condenser we cannot see what takes place.

As long as the instrument was made with metallic substances only, we were obliged to confine ourselves to the exterior marks of inflammation alone, without being able to assign the true cause, or at least furnish proofs of it. For to guess is not sufficient in natural philosophy; we must demonstrate, in order to give to facts that degree of certainty, which befits science; and this we cannot do here, without seeing what passes at the very point of inflammation.

We can in a glass tube.

(Glass flutes.)

The means are very simple. Nothing is necessary, but to substitute a glass for a metal tube. Those found in the shops being too slight, I applied to Mr. Laurent, the inventor of glass flutes, requesting him to procure me tubes of a similar quality. This artist, as much distinguished by his civility as by his talents, furnished me with three, which I fitted up. The first, eight inches long by eight lines in diameter, did not kindle the spunk. The second, nine inches long by six lines and three quarters in diameter, kindled it completely. This being destroyed by accident, I tried the third, eight inches long by seven lines in diameter, which succeeded equally well.

Appearances.

A bright light if the compression be rapid:

When the instrument is made to act, and the spunk kindles, we see a bright flash, that fills the capacity of the tube; and this light is so much the more vivid, in proportion as the compression is more rapid. If the compression be less powerful, the spunk does not kindle, but we perceive in the upper part of the tube a light vapour, that falls in undulations on the piston. When this has disappeared, if we draw back the piston, the vapour will reappear, as long as there is any air in the tube. These effects may be produced several times in succession, merely by pushing the piston with the hand. This vapour is so thin and diaphanous, that it is not perceptible in a strong light. It requires a sort of twilight to see it well.

a light vapour, if not.

Whence arises this vapour?

But whence arises this vapour, and what is its nature? Assuredly it is not furnished by the materials of the instrument; it can only proceed, therefore, from what it contains, from the atmospheric air. Now, according to the present state of our knowledge, the air contains only nitrogen, oxigen, and a very small portion of carbonic acid; all gasiform substances, which are kept in this state by the great quantity of caloric that penetrates them, and are consequently heavier than it*. But in compressing the air contained in the tube, what is the substance that must first give way? Is it not that which is lightest, the caloric, that general solvent, that principle of fluidity and volatilization, which gives wings even to metals to raise themselves in the air? Is then the vapour in question caloric, rendered visible by the approximation of its particles, which are compressed by the surrounding air, as air becomes visible in passing through liquids? This idea, which I am far from presenting

Is it the matter of heat condensed?

It is not aqueous vapour.

* The air likewise, in its ordinary state, contains twelve grains of water in a cubic foot. This small quantity of water, reduced to the proportion of the quantity of air contained in the apparatus, contributes nothing to its effect: for the heat produced by the friction could at most reduce it to vapour, and in this state it would not kindle the spunk. If the vapour seen in the tube were water in a state of expansion, when it fell on the surface of the piston it would condense there, and appear in the state of a liquid. But the surface of the piston always remains dry, though on moving it the vapour appears and disappears several times.

as a thing proved, acquires more probability from the following experiments*.

Trials with other gasses.

I substituted hidrogen for common air, and the vapour showed itself as before; but the spunk did not take fire. With carbonic acid gas, and with nitrogen the effects were the same. The latter, which contained a little nitrous gas, gave a somewhat denser vapour. Oxigen, lightly compressed, yielded a vapour more rare and transient than that from common air. It had scarcely fallen on the piston, when it rebounded and disappeared. When I compressed oxigen with a proper force for producing inflammation, the spunk, which commonly takes fire only at the anterior part, was almost entirely burned: yet for this experiment I used a copper instrument, the piston of which lost air so much, that it would no longer kindle spunk [with common air].

The vapour cannot arise from the grease employed.

Perhaps it will be said, that the vapour came from the greasy matter on the piston, which adheres to the sides of the tube; and that it is expanded by the heat produced by the friction. To this I answer, in this case, 1st. The vapour should not show itself before the greasy matter is deposited on the sides of the tube; yet it appears at the first stroke of the piston, before the tube becomes greasy. 2dly, It should show itself below the piston, in the part which the piston has left; but, on the contrary, it always shows itself above. 3dly, There is no vapour, when the piston loses much air, if the friction be ever so rapid. 4thly, The vapour should be more apparent, when the piston exerts its friction throughout the whole length of the tube, than when it is confined to a small part of its upper extremity; yet the reverse frequently happens. 5thly, When the air is entirely decomposed no more vapour appears, but it shows itself again, if ever so little fresh air be introduced.

It contains no acid.

As it was essential to ascertain whether the vapour did not contain an acid principle, I fastened to the surface of the piston, with a little green wax, a piece of muslin dipped in infusion of litmus, and afterward dried. After twenty strokes of the piston the colour was not changed. I put on a second piece of muslin larger than the first, and the edges of which were loose.

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* Mr. De Luc ascribes the ignition to the condensation of the matter of heat. See Journal, vol. XXI, p. 234.—C.

This was burned all round, without the colour of the rest being altered. Lastly, a third piece, which was wet, experienced no change of colour.

General inferences.

From these experiments it follows, that no acid principle is developed: that all aeriform substances, as well as common air, produce a light vapour: that no other gas, except oxigen and common air, kindles the spunk: that oxigen produces a much more powerful combustion than common air, consequently oxigen acts an important part in the inflammation: that as it can exert its action only when set free by the decomposition of the common air, of which it constitutes a fourth part, it follows, that the air contained in the tube is decomposed by the simple force of compression: that the vapour produced is not owing to the oxigen, since it shows itself equally in gasses that contain no oxigen: that this vapour is the effect of some agent common to all gasses: and that we may presume it is caloric itself, rendered visible by the sudden approximation of its parts in a small space, where it rises to a temperature that is increased in the oxigen so as to kindle the spunk*.

Luminous meteors independent of electricity,

I am equally induced to believe, since the air, and it is the same with all gasses, is decomposed by rapid compression, that the luminous meteors frequently perceived in hurricanes are not always the effects of electricity. I have observed several times, on these occasions, that Saussure's atmospheric electrometer affords no signs of any. I will mention a particular instance, as it occasioned me no less surprise than damage.

in high winds.

In the beginning of the year 1803, being at my country seat, toward evening a violent wind arose, which continued increasing for two hours to such a degree, as to blow down about sixty trees of prodigious size and height in an ornamental plantation. It threw them one upon another in a row, and some of them were broken off. Those that were torn up by the roofs brought up the earth with them to the distance of

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Vapour of a different kind.

* It sometimes happens, that the spunk is turned black without kindling. In this case, as well as when it is kindled, if we draw back the piston in the tube, a dense vapour, that may be smelt, issues out, which is not of the same nature as the former. That shows itself before the inflammation; this always succeeds it. That is the principle of the inflammation: this a product furnished by the combustion of the spunk, of which it has the smell.

fifteen feet. The clouds flew with extreme rapidity, and twice I saw flashes of light from them. I raised my electrometer, armed with its conductor two feet long, but the balls still continued in contact.

If these researches afford nothing more than conjecture, they will have at least the advantage of serving as a guide to more enlightened observers, whose labours may extend our knowledge of a very obscure subject, to elucidate which is difficult.

XIII.

Analyses of Minerals. By Martin Henry Klaproth, Ph. D. &c.

(*Concluded from vol. XXXII, p.* 384.)

Magnesite.

MAGNESITE from Styria*,

Magnesia	48
Carbonic acid	49
Water	3
	100

Magnesian limestone.

Gurofian (so named by Karsten from the place where found).

Carbonated lime	70 5
magnesia	29·5
	100

Wavellite from Barnstable,

Wavellite from Barnstable, in Devonshire.

Alumine	71·5
Oxide of iron	0·5
Water	28
	100

and South America,

Wavellite brought from Hualgayoc in South America, by Humboldt.

Alumine	68
Silex	4·5
Oxided iron	1
Water	26·5
	100

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* For a paper on this stone by Messrs. Haberle and Bucholz, see Journal, vol, XXXI, p. 269.

Siliceou

Siliceous guhr from the Isle of France.

Siliceous guhr.

Silex	72
Alumine	2·5
Oxide of iron	2·5
Water	21
	98

A green mineral, having the appearance of a sandstone, from Spessart.

Green sandstone.

Silex	85·25
Alumine	1
Oxide of iron	7
Water	5
	98·25

Hepatite from Andrarum.

Hepatite.

Sulphated barytes	85·25*
——— lime	6
Oxidulated iron	5
Alumine	1
Charcoal	0·50
Loss, including water and sulphur	2·25
	100

Botryolite†.

Grapestone.

Silex	36
Lime	39·5
Boracic acid	13·5
Oxide of iron	1
Water	6·5
	96·5

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[*] In the J. de Phis. 25· 8. This being evidently wrong, the figures probably had fallen out and been replaced erroneously by the pressmen; and, as the loss is noticed, the whole sum should of course be 100. From the appearance of the figures too, there being a vacancy for one, I have little hesitation in correcting it as above.—C.

[†] See Journ. vol. XXVI, p. 273, for a description of it.—C.

Zircon

Jargon.

Zircon from the Circars, in the East Indies. Sp. grav. 4·5.

Zirconian earth	64·5
Silex	32·5
Oxide of iron	1·5
	98·5

Red garnet from Greenland.

Red garnet from Greenland.

Silex	43
Alumine	15·50
Magnesia	8·50
Lime	1·75
Oxide of iron	29·50
——— manganese	0·50
	98 75

Kannelstein.

Kannelstein*.

Silex	38·80
Lime	31·25
Alumine	21·20
Oxide of iron	6·50
Loss	2·25
	100

Opake black tourmalin of Haüy.

Common schoerl from Eibenstock.

Silex	36·75
Alumine	34·50
Magnesia	0·25
Oxidulated iron	21
Potash	6
Oxide of manganese, a *trace*	
	98·5

Opake black tourmalin of Haüy.

Common schoerl from Spessart.

Silex	36·50
Alumine	31
Magnesia	1·25
Oxidulated iron	23·50
Potash	5·50
Oxide of manganese, a *trace*	
	97·75

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* This appears very different from the kannelstein analysed by Lampadius. See Journ. vol. XXIII, p. 281.—C.

Common hornblende from Nora.

Lamellar amphibole of Haüy.

Silex	42
Alumine	12
Lime	11
Magnesia	2·25
Oxidulated iron	30
Manganese	0·25
Water	0·75
Potash, *a trace*	
	98·25

Basaltic horneblende from the country of Fulda, found in volcanic basaltic substances.

Supercompounded crystalized amphibole of Haüy.

Silex	47
Alumine	26
Lime	8
Magnesia	2
Oxidulated iron	15
Water	0·5
	98·5

Common black augite from Rhorgebirge, in Franconia.

Black augite from Franconia.

Silex	52
Lime	14
Manganese	12·75
Alumine	5·75
Oxide of iron	12·75
——— manganese	0·25
Water	0·25
Potash, *a trace*	
	97·75

Common green augite.

Virescite of Delametherie.

Silex	55
Magnesia	13·75
Lime	12·50
Alumine	5·50
Oxide of iron	11
——— manganese, *a trace*	
Water	1
	98·75

Black augite from Frascati. Pyroxine of Haüy.

Black crystallized augite from Frascati*.

Silex	48
Lime	24
Magnessia	8·75
Alumine	5
Oxide of iron	12
——— manganese	1
Potash, *a trace*	
	98·75

Black garnet.

Melanite†.

Silex	35·50
Lime	32·50
Alumine	9
Black oxide of iron	24·25
Oxide of manganese	0·40
	101·65

Gadolinite.

Gadolinite from Bornholm.

Yttria	60
Silex	22
Oxidulated iron	16·5
Water	0·5
Oxide of manganese, *a trace*	
	99

Fettstien.

Elæolite‡.

Silex	46·50
Alumine	30·25
Lime	0·75
Oxide of iron	1
Potash	18
Water	2
	98·5

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* See Journ. vol. XXVII, p. 148.

† Ib. p. 151.

‡ See a description of it, Journ. vol. XXVI, p. 384.

Apatite.

Apatite in mass from Uto.

Phosphated lime	92
Carbonated lime	6
Silex	1
Oxide of manganese, *a trace*	
Loss in roasting	0·5
	99·5

Bituminous slate.

Brandschiefer, or bituminous schist, from Wologda. 200 grs. yielded

	c. inch.	grs.
Sulphuretted hidrogen gas	80 =	28·8
Empyreumatic oil		30
An oil as thick as pitch		5
Ammoniacal water		4
Charcoal		20
Silex		87·5
Alumine		6·5
Lime		10·5
Magnesia		1
Oxide of iron		3
		196·3

Water of the Dead Sea.

Water from the Dead Sea. Sp. grav. 1·245[*].

Muriated magnesia	24·2
——— lime	10·6
——— soda	7·8
Water	57·4
	100

Sulphate of zinc.

Crystallized vitriol of zinc, from Rammelsberg.

Oxide of zinc	27·5
——— manganese	0·5
Sulphuric acid	22
Water	50
	100

[†] For a very full account of an analysis of this water by Dr. Marcet, see Phil. Trans. for 1807, Part II; or Journal, vol. XX, p. 25.—C.

Red silver ore. Rothgultigerz.

Silver	60
Antimony	19
Sulphur	17
Oxigen	4
	100

Ore of lead. Fibrous arseniated phosphated lead, from Rosier, near Pontgibaud, in Auvergne.

Oxide of lead	76
Phosphoric acid	13
Arsenical acid	7
Muriatic acid	1·75
Water	0·50
Loss	1·75
	100

Ores of titanium. Iserine.

Oxidulated iron	72
Oxide of titanium	28
	100

Granular titanited iron.

Oxidulated iron	85·5
Oxide of titanium	14
——— manganese	0·5
	100

Pitchlike iron ore. Ferruginous pecherz, or piciform iron, from Freyberg.

Oxide of iron	67
Sulphuric acid*	8
Water	26
	101

Ironglance. Octaedral crystallized volcanic eisenglanz. Sp. gr. 3·88.

Oxidulated iron	66
Silex	29·50
Alumine	4
Potash	0·25
	99·75

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* So in the French.—C.

Tin pyrites.

Tin pyrites.

Copper	30
Tin	26·5
Iron	12
Sulphur	30·5
	99

Realgar.

Realgar.

Metallic arsenic	69
Sulphur	31
	100

Orpiment.

Orpiment.

Metallic arsenic	62
Sulphur	38
	100

Sphene from Salzburg.

Ore of titanium.

Oxide of titanium	46
Silex	36
Lime	16
Water	1
	99

Meteoric stone that fell at Lyssa, in Bohemia, the 3d of September, 1808*.

Meteorolite of Lissa,

Iron	29
Nickel	0·50
Manganese	0·25
Silex	43
Magnesia	22
Alumine	1·25
Lime	0·50
Sulphur and loss	3·50
	100

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[*] See Journal, vol. XXXI, p. 224.

Smolensko, Meterolite that fell in the government of Smolensko, the 15th of March, 1807*.

Iron	17·60
Nickel	0·40
Silex	38
Magnesia	14·25
Alumine	1
Lime	0·75
Oxide of iron	25
Sulphur, manganese, and loss	3
	100

and Stannern. Metereolite that fell near Stannern, in Moravia, the 22d of May, 1808†.

Silex	48·25
Alumine	14·50
Lime	9·50
Magnesia	2
Iron	23
Loss, including sulphur and manganese	2·75
	100

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[*] See Journal, vol. XXV, p. 59.

[†] See Journ. vol. XXXI, p. 229; and for analyses of this stone by Mr. Moser and Mr. Vauquelin, vol. XXV, p. 55 and 58.

Crystallographic Models, exhibiting the forms of Crystals, their Production, Geometrical Structure, Transitions of Forms, and mechanical Dissections. Intended to illustrate the Science of Crystallography, after the Method of Haüy. *Accompanied with a Treatise elucidating the Elements of that Branch of Knowledge.* By FREDERICK ACCUM, M. R. I. A. Operative Chemist and Lecturer on practical Chemistry, and on Mineralogy and Pharmacy.

"*In future* the name of God, will be as distinctly written on a crystal, *as it has* hitherto been in the Heavens." Philosoph. Journal, vol. ix, p. 87.

To Mr. Nicholson.

SIR,

Cultivation of mineralogical science.

THE general attention which of late years has been paid to the science of minerals cannot have escaped the notice of the most superficial observers. No department of natural history has been cultivated with more ardour and success than mineralogy, and in none have the cultivators of science been more numerous, both at home and on the Continent. It embraces a wide circle of votaries among the curious and wealthy classes of the community, and it is intimately connected with that laudable passion for exploring the productions of nature which characterises the age in which we live.

Symmetrical solids or crystals.

Indeed, under whatever points of view we examine the shell of our globe, we are struck with the variety of its productions. When we cast our eye over the substances which compose the collections of mineralogists, or the cabinets of the curious, we behold a vast number of bodies, which are regularly shaped, and exhibit the forms of geometrical solids. The substances are called crystals.

General facts respecting crystals. Varieties in the same substance.

When we examine the constitution of crystalline solids by the methods of chemistry, we become convinced, that the same identical substance, or material, does assume different figures, which frequently bear no such resemblance to each other, as would seem to indicate their relation. And chemistry, or the chemical art, is also capable of causing bodies to assume symmetrical forms; and the figures of these are likewise liable to be altered by circumstances, which affect the crystallizing process. Sugarcandy, for example, usually crystallises in oblique four-sided prisms with wedge shaped summits; but it is also met with in six-sided prisms variously modified. Alum crystallises in octahedrons, but it likewise assumes the shape of a cube. It is found nevertheless, that a certain number of figures are peculiar to each particular crystallisable material; and the crystals of that substance assume some one of those forms, or their modifications, and no other.

The elementary parts are disposed in symmetry according to laws

This however is not all. When we penetrate into the interior structure of these solids, we become convinced, that their mechanical elements are symmetrically placed according to certain laws, which have their measure and their value. Their aggregation is absolutely geometrical, and appears as if it had been effected by instruments guided by skill and intelligence.

—deducible mathematically.

To exhibit these laws of crystalline architecture, is the province of crystallography. This science has in our time been so successfully cultivated, that it gives a dignified aspect to the philosophy of minerals, as grounded upon the results of the most elaborate and skilful analysis. By these we are enabled to cálculate with the fewest possible data, simple in the extreme, and mathematically certain, the vast variety of forms of crystals, with a like degree of accuracy as astronomers attain in calculating the motion of the heavens.

Great advantages of models for explaning this doctrine.

But as the knowledge of crystallography in its improved state abounds in mathematical and algebraic calculations, and cannot therefore be studied with success by such as are unacquainted with the mathematics; it has been proposed to illustrate its elements by the help of geometrical models, which, in other departments of knowledge, are so singularly useful in rendering mathematical demonstrations obvious to the senses. Undoubtedly the human mind is capable of receiving information from the mathematics with much greater facility for demonstrations afforded by tangible solids, than by mere reasoning from designs drawn upon a plane surface. It requires an eye familiarised with the rules of linear perspective to comprehend the diversified and often complicated forms of angular polyhedrons represented by projections by straight lines only, which must naturally cross each other in many directions in the representation of crystalline bodies.

Advancement

The general advancement of science and arts must be greatly dependant on the facility with which their practical means can be obtained. Less than thirty years ago there were not three places at which the ready prepared materials of philosophical chemistry could be purchased in this great metropolis. There was but one maker of turning lathes; philosophical instrument makers were very few; and there were no steam engine makers, agricultural implement manufactories, &c. in London, with which we now so plentifully abound. I would submit to your consideration, Sir, that he who establishes a place of fabrication or deposit of an article of use to the sciences, which could not before be purchased, is a benefactor to the public; and under this point of view, I offer you the present notice, as a piece of scientific news, though it is likewise of a private commercial nature. I have, with considerable expense and attention, prepared a set of models of crystals, partly solid and partly dissected; and have made arrangements, which enable me to supply the public. The dissected crystals are so constructed, that they can readily be taken to pieces and built up again in various ways, to give the untutored eye a distinct conception of the laws of that geometry, which are followed by the integrant particles when they combine, and the orderly arrangement of which produces symmetrical crystals. And this in fact constitutes the basis of the science.

of science from the completion and sale of the useful means of practice.

Dissected models by the author.

A single glance at the dissected models will enable the student to comprehend why crystals are always rectilineal bodies, bounded by planes; and whence that immense variety of actually existing crystalline forms is derived, with which the mineral kingdom has hitherto astonished the world.

I have likewise composed a treatise, which will form a work distinct in itself; but nevertheless so composed, that it may serve as an index of reference to its models through the work*.

Treatise referring to the models.

And as the series of solids to be finished on the present occasion will be limited, such individuals, who are desirous of receiving sets of them, will have the goodness to favour the author with their orders, either in a direct way, or through the medium of their booksellers.

Subscription proposed.

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* This work, which is in the press, will shortly be published by Messrs. Longman, Hurst, Rees, Orme, and Brown, Paternoster-row. The copper plates for the work are engraved by Lowry; and the lineal projections by Berryman and Brandstone.

This condition is essential, because the author presumes he could otherwse employ his time and labours with more advantage to himself, and the public. Farther information may be had at the laboratory, where several thousand models, both solid and dissected, are ready for inspection.

I am, Sir,
Your obliged servant,
FREDERICK ACCUM.

Old Compton Street, Soho,
October 25th, 1812.

Queries. By Inquisitor.

To Mr. Nicholson.

SIR,

Queries in natural history.

THE following queries are submitted to the scientific eye of the perusers of your Journal, and an answer solicited by
INQUISITOR.

QUERIES—Is there a species of lichen, or of any other cryptogamin plant, in the form of a powder, of a light azure colour?

Or do the ova of any insect, or the insects themselves, exhibit this appearance?

Where is the description of such plant or insect to be found?

☞ *For want of room, the accounts of Kirchoff's discovery of a process for converting starch into sugar; with the experiments and inductions of other chemists, are deferred to our next.*

Dr. Pearson's reply to Dr. Marcet came too late for the present month.

The communication and drawing from Mrs. Ibbetson came duly to hand, and will be published in our next number.

ERRATA IN THE LAST NUMBER.

P.	L.		
146	18	For "*rectrified*"	read *rectified.*
	27	"flʒij to flʒiv"	flʒij to flʒiv.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER, 1812.

ARTICLE I.

On the Growth or Increase of Trees: by Mrs. AGNES IBBETSON.

To Mr. Nicholson.

SIR,

I HAVE before proved, that there is a vital principle in all plants, from which all flowers proceed; from which the seed is formed, and from which the interior bud is protruded. I have also shown, that in all plants which rise yearly from the earth, whether annual or perennial, the buds shoot from the root; but in all trees and shrubs, from the nearest line of life, which is that *vital part* adjoining the pith. The next matter of importance to the development of the nature of trees is to know and understand, as exactly as possible, how they *increase in size*. That the wood is enlarged by an additional cylinder each year, we are well apprised; and that a new shoot is formed each spring and autumn, we also know: but here our knowledge ends. No one has ever attempted to inquire in what manner that stripe is added, or what preparations nature makes for the purpose: satisfied with the result, they seek no farther, though, without knowing it, the *existence of a tree* is

To show the manner of the increase of a tree.

unintelligible to us. Nor are we informed how nature, in so hasty a manner, can protrude such a length of shoot as is often seen, in the autumn particularly. And yet all this is of the utmost importance to be known; it is that leading ray, which should enlighten all the rest, and give a more perfect perception of the formation of that extraordinary production called a tree, which perhaps may be truly said to collect within itself more wonders than any other matter whatsoever, and which nothing but the custom of viewing daily could enable us to see without constantly increasing astonishment; a being endued with life, and yet governed by mechanical powers; capable of selecting from the juices of the Earth the quantity of sap necessary to its increase, and yet drawing only that, and adapting its increase to the quantity drawn; elaborating its own juices, and by this means rendering them more suitable to the tender existence of the new bud, fit to invigorate the flower, and prepare it for the perfecting the seed; enabling it, by mechanical means, to support its leaves, that no rain water may drop from them on those below, which, if not provided against by nature, would soon putrefy the lower part of the foliage*; but by their varied motion, and mechanical action, so manage, as to throw off to a distance the water thus gathered: enabling the leaves to turn in such a direction, that each may partake of that light absolutely necessary to the welfare and health of the whole, and though producing a deep shade for the solace and refreshment of man, yet each leaf capable of placing itself so as to receive rays of that vivifying matter, light, which we every day learn is more necessary, not only to the animal creation, but also to the vegetable world.

The mechanical powers of a tree.

For a considerable period my time has been dedicated to the studying the new shoot in trees, watching its daily progress, marking it with threads, and then dissecting it in various states of augmentation. By these means I have, I flatter myself, gained a tolerably perfect knowledge of the whole proceedings, and no part of botanical physiology is more worthy a minute inquiry. It is an easy matter to establish a beautiful theory to captivate the imagination, though without elucidating a single fact: but to understand every part of the formation of a plant, interior as well as exterior; to dissect and watch its various states and changes; to examine thoroughly how it passes into each, and what has been the general effect produced in the vegetable by such alterations; to collect by dissection and by culture its habits and powers—these are the requisites, and all this must be gained by examination and study, before we can at last form a theory founded on truth, and learn to know what a plant really is. This is my aim, and upon this I have already employed fourteen years of the most unremitted application. I shall now show the manner of a tree's increase in every way; how the spring and autumn shoots are protruded; what is the difference of various trees in this respect, and also the changes produced in the new shoot, when compared with the older parts of the tree; how the yearly stripe in the wood is contrived, with many other particulars, as they may occur to me.

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* The contrivance selected by nature to enable the leaves of trees to throw off the rain water which is not necessary to them, is to be found in the gatherers: they have moments of shaking, which seems to be caused by some sudden effect of the spiral wire. I have repeatedly placed a paper windmill to ascertain whether it was the effect of wind, and found it not so.

Necessary manner of study.

The screws which indicate the shoots.

If a tree be examined about the beginning of August, it will be perceived, that a sort of screw is forming at the end of the last year's shoot. Each different tree has its own peculiar screw, appertaining to the whole genus. Thus, in the poplar it is long and scattered; in the oak, short and close. It is found by many deep rims, which are partly the outward marks indicating the bud, but perfectly divided all across the plant, one from the other, within as well as without. When you take off the bark and rind from the screw, you find the interior wood swelled with the buds of the year, which are to develop the next spring, and will then be arranged and placed in the bark of the screw. It is now that you see, in the most pointed manner, all I have before shown respecting the bu[illegible]; viz. that the wood vessels open and disperse to let them out; that the buds possess all this time no other covering than a few coats of alburnum, and have no scales till they reach their cradles in the bark; and that it is the thickened form of this bark in each screw, which allows of the concealment of the buds, where they remain till they have woven their scales or winter covering. I have said, that the screw is a collection of rings or links; there is also a part

The shooting of the screw.

The new shoot differing from the old.

Shooting without screws.

a part attached to each, capable of increase, and which draws out like a telescope; this increase is generally the usual distance allowed to new shoots in every sort of plant between bud and bud, and of course varies according to the tree. When the screw is formed, and the buds arranged in each, then the shoot begins to push; and here again great variety is discovered—in some trees a quarter of a screw divides, and then runs up to the end of the shoot, forming a long distance between the buds in each new division, and thus continues to develop till all the different *links* are expanded. In the horse-chesnut it will separate into various pieces; its leaves and buds growing from each extremity, equally developing it both ways; but it may always be known which link, or which *part of the screw* is drawing out, by the youngest leaves being in *that place*. In the ash, nearly half the screw first shoots up to the termination of the new branch, and then continues to unfold that piece of the screw, till it is all expanded; it then completes the formation of its *winter bud,* and when *that* is once *protruded* in a plant, it never shoots a piece beyond it *that season*. This is the case, I believe, in every forest tree: in all trees the new shoot differs from the rest, not only in the manner of placing its leaves, but in the appropriate distance of the buds. In the oak the leaves are alternate, and there is not half an inch distance between them: but in a new shoot two leaves come out almost opposite, or within a quarter of an inch, and then pass on a full inch before they recommence their former progress. The first shoot of the elm is very different; the leaves are all twice the size of any other in the tree, and the distance of the leaves is in proportion. Also the screw almost always develops below, at the part where the new shoot begins, and very rarely at the extremity of the branch:· besides, nature establishes a curious difference between shooting from an embryo, when a tree is first formed, and pushing its half-yearly branch in spring and autumn. In the first, while yet in the seed, it forms many buds, and while it is developing its seminal leaves, many more are added to the number: from this preparation the branch shoots at once, without waiting to arrange them in screws, because they then may be said to shoot, like an herbaceous plant, from the root, which of course they can never do afterward; and must be protected by the sheath the leaves always lend to young plants of every kind. It is wonderful to see how nature adapts her proceedings to the case in point, and how you may make her vary her modes by changing or altering the situation to which she is exposed. This is the reason that makes me so unwilling to trust to any knowledge gained by placing a plant in an unnatural position, which is certainly the case when we stop the sap; arrest the flow of the blood; varying the growth of any particular part, or play any tricks of that kind in order to benefit by the means she will adopt to right herself; but we are not *enough* acquainted with the *whole arrangement* of plants, to improve by such a mode of practice: the result is only formed to lead us into errour; we misapply the cause, and build a theory on falsehood. The only proper way of studying plants is *constant watching* and *dissection*. The person who will not give up *some years* to the study, should not attempt it—but, to return to my subject. It is not only the shoots from the embryo that come up without a screw; it is the case also with *hungry branches*; these hasty productions are seldom seen in forest trees, though minor trees and shrubs are very subject to them. Whatever part of the tree may be the base from which these branches shoot, a quantity of buds is first formed at that place; and it is, perhaps, this very cause, that makes them run up so hastily. The buds being ready, they soon appear at the extremity of the twig, one by one, till they have expended all that were assembled—it is the same also when a stool is hewn for procuring trees, or when a pollard is fresh cut; the large space allows room for such a number of buds to form, that it appears no longer necessary to arrange them in that exact manner; but they run up hastily, and are soon seen rising alternately at the end of the twigs, and developing both buds and leaves. I have sliced several pollards and stools in this situation, just as they were going to shoot; and the buds have so crowded on each other, that it has been absolutely impossible to count their numbers. This manner, however, of shooting never takes place above once; the second time always comes with the screw as usual.

Necessary to give much time to the study.

Manner of shooting in stools and pollards.

I shall now show how the yearly increase of the stripe in the wood is contrived, which forms the horizontal addition to a tree in width: it is, if possible, attended with more curious circumstances than the increase of the new branch—but I know not any thing more difficult to discover, or that has cost me so much trouble to gain; as it requires so perfect a knowledge of the formation of the tree, and the general disposition of the several parts in each different wood:—but the dissecting and comparing the shoots of autumn and spring, by fresh vegetable cuttings, and watching in trees their increase, has at last enabled me to effect it; and it will be much *more easy* for a person to follow me, now the *matter is known*, than first to make the discovery. Choose a tree of any kind that you can cut to pieces, take off a large branch near the stem at the beginning of August; between the wood and bark a row of alburnum will be found—it is distinguished by being of a clearer and softer substance than any other in the tree: it is this alburnum which is deposited each season, half a circle at a time, and which the next season becomes wood. You will then find the bark and rind are retired back at the south side of the tree, leaving a diminutive space between the alburnum and bark, which is preparing for the season's increase. It is this which causes them to be so easily severed, and makes this the proper season for barking. Take a vegetable cutting of the branch, and examine the alburnum in the solar microscope; it will appear perfectly clear and free from all vessels, and to be merely what I before announced it, a jelly of sap. Continue to cut fresh specimens, and display them daily before good magnifiers, and they will soon show the sap-vessels beginning to run through this stripe of alburnum, and the bastard vessels shooting also across it, but in a contrary direction. In a fortnight's time, that part which was alburnum is now become perfect wood, and the jelly of sap will appear to be forming beyond it, filling up that place from which the bark had receded for the purpose, and forming a new circle of alburnum, which the next autumn, in its turn, will be converted into complete wood. This must at once show how the wood and bark are protruded in trees; and end that eternal dispute, whether the bark make the wood, or the wood the bark. It is certain, that they are of a totally different nature, and yet in one respect agree in their formation. That it is the juices which form the softer part of each; that these coagulate, and then wait for the growth of the separate vessels, which shoot out vessel within vessel, thus lengthening as necessary, and protruding

Yearly increase of sap.

truding like the new shoot of the spring and autumn, and drawing out like a telescope. This is the manner in which the wood vessels increase; the bark vessels are rather different, as I shall explain at another opportunity. But this is not all which is of consequence to the subject—the retiring of the bark-vessels to make way for the new row of alburnum is managed in various ways in different trees. In most fruit trees the bark-vessels bend up, receding from the part the alburnum is to occupy, and then pushing out towards the rind, and thus increasing the circle. In forest trees the smaller cross vessels break away, and leave all the circular ones to retire towards the rind. But whichever way they act, I have a specimen which elucidates each fact, and makes it beyond contradiction: and it may easily be seen, that constantly taking the cutting of a branch every season from the same tree, its increase, and the manner of it, must be exactly noted: but it sometimes happens, that the season is unfavourable, and that the severity of the weather so checks the sap that should form the new row of alburnum in March, that it rises not sufficiently to deposit so large a semicircle; then the old remains on that side, and causes that appearance sometimes found in wood, which presents the yearly circle incomplete; but it occurs not often, especially in *indigenous trees.* Nature performs her part too perfectly, unless we make her fail by removing various trees and shrubs from a more favourable climate to our own—then I have seen it produce a *strange effect.* I have many specimens, in shrubs particularly, where the pith has been wholly on one side, almost joining the bark, though twenty or thirty circles, well defined, have shown three quarters of the year; but the winter quarter has been as void, as if it should never have had a mark—this must be wholly owing to its missing its spring shoots, from the coldness or damp of our climate. The exact manner, in which every branch in a tree tells its own age, is also a curious fact. I have before observed, that the trunk of a tree shows exactly how long it has been planted, but the branch shows only the seasons it has grown, one row for each year. I have taken a whole tree in this manner, examining each division; and the exact way in which it answers to the time of its shooting is curious to see—the autumn shoot, however, is so much wider than the

The retiring back of the bark.

The pith seldom in the middle of a tree.

the spring*, that the pith is seldom in the middle of the trunk, except the tree is very strong, and in a very sheltered situation. But there is another point worthy of attention. If a deep indenture is made in a tree, the mark will go on increasing as long as the tree continues to grow, just as in a range of circles, an angle increases from the centre to the circumference. Thus, if I form a cross on the stem of a tree, twenty years hence that mark will show exactly what increase that stem has made in the middle; and, by the number of coats laid on it in the wood part, how many years since it was first indented. But it certainly appears, on first consideration, most wonderful, that it should do so, considering the extreme change each fibre undergoes, and how often every part must be moved to let the buds pass out from the interior; but on examining a tree *barked*, the miracle ceases—when once an impression is made, every succeeding cylinder is so contrived, that it must enlarge the mark by the progressive motion of the parts, the very thin layers that are added each year, and the forcible and perpetual compression the whole undergoes. It is the same with many natural marks formed by the missed buds or bulbs, the existence of which I have before shown, or any other accidental impression in a tree. This natural effect was productive of a very curious consequence during the time of irreligion and riot in France. A poor widow cleaving a tree to procure some fire-wood to sell, found the mark of a cross in the interior of the trunk of an ash—she never looked at the rind to seek a correspondent impression, but took it for a miracle, a declaration of the Almighty. All the people crowded to see it; the widow was soon enriched, and it had a better effect on their morals, than all the edicts in favour of religion afterward promulgated by Bonaparte, or the horrid experience of times divested of all piety.

The effect of cutting marks on a tree.

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[*] It is astonishing, how many exotic shrubs, and even trees, grow only in the autumn, and miss their spring shoots, and have, therefore, the pith quite on one side. I have traced this in a number, and, by taking them at the proper time in several different specimens, secured the most absolute proof, that this is the manner in which they increase: this truth, therefore, like the coming out of the bud from the interior, cannot be denied, since no tree or shrub can be examined, without proving it.

But

No torpidity in wood.

But that wood ever so old should get into that torpid state described by some botanists, is certainly a very gross mistake: as soon as the sap ceases to flow, the pipes decay, the rot is introduced, and death ensues: for when all motion ceases in the wood, it can no longer divest itself of those minute parts, which, accumulated, would soon cause its ruin. There are little fibres which join together the bastard vessels, and are constantly renewed every [illegible] years, decreasing in length as the compression of the wood makes it necessary. Their motion, therefore, in letting out the buds also divests them of their *extraneous matter*, which would otherwise fill up the places left for the new shoot of alburnum: but let the age be ever so advanced, the stems will throw out new branches, the line of life new buds. I have a log of a tree adjoining the trunk, with above ninety yearly circles—where there are two or three large buds protruding, and the wood vessels making way, as in quite young trees: but that there is some age at which the wood ceases to form in width, there can be no doubt. I think I have traced its manner of proceeding in this respect—but I have so seldom an opportunity of gaining a fit specimen from a very old tree, to ascertain the truth, and am so unfortunately circumstanced for procuring any thing of the kind, (although ever so much wanted,) that few would have the courage to study, so surrounded with obstacles. In a specimen known to be between two and three hundred years old, I have got a vegetable cutting: for eighty years it proceeds in the common manner—then the rows increase, not in the usual place, but *between* the *others*, forming five between each row—this continues for near thirty years; then it passes on to the old place between the bark and the wood, and increases only on the south side of the tree *each autumn*, without any sort of addition in the *spring*, or *north quarter*. This goes on about sixteen or eighteen years, when an entire stop to the growth seems to take place in *width*—it may then be supposed, that the tree, having attained its perfect size, stops for a certain number of years, and then begins to decline, still throwing out buds and branches, and never too old for this, since the oldest possible tree, if freed from rot, and having the *exterior pared*, and a plaster put on, would form new wood, and generate a quantity of buds. I have tried this in such extremely old subjects, that I am convinced it is part of

Get rid of extraneous matter.

Shooting on the south side only.

the wonders of the vegetable; and that if, therefore, trees were taken care of, they would die only at a very old age. But few of our trees are allowed to gain maturity: we, indeed, use them so shockingly ill, that there is no chance of their reaching to such a time of decay. If noblemen and gentlemen, instead of planting such a number of trees, would lessen the number, and take care of those growing—be as saving of them as of their game—make it the business of the land steward, or bailiff, or gamekeeper, to see that no trees are damaged, or allowed to decay before their time—that the unprofitable branches are lopped, the cankered arms cut off, the withered tops curtailed—that the trunk is not allowed to form holes, or to split, without being joined and plastered—that they are, when first growing, cleared, the sun, air, and wind admitted to them (for to this last they owe their being saved from vermin and blights)—But I mean not now to enter on a farther discussion of this matter—my present subject is not the preservation, but the increase of the tree, which strictly examined by the rules, and in the manner I have advised, will, I flatter myself, be found exactly conformable to truth, and delineated with as much precision as the difficulty of the subject will permit.

Ill usage of trees.

I am, Sir,
Your obliged Servant,
AGNES IBBETSON.

Description of the Drawings.

Drawing to illustrate the manner in which trees increase in growth.

I shall now give the drawings, grieved that it is not in my power to show them in their natural state; for to argue from living specimens at once makes all contradiction impossible, and is as delightful to the teacher as to the instructed. Pl. VI, fig. 1, is the screw of the beech with the winter bud already formed: it is much magnified, and the three leaf-stalks show the manner in which the old branch shoots in the beech, while fig. 2 is the way the new shoot throws out its leaves; in the old parts the leaves are in threes or fours; in the new shoot the leaves are always alternate, with a distance of an inch and a half between. Fig. 3 is part of the same branch laid open, with the buds in their cradles, and with the divisions that show completely how they are to shoot at BBB: and CCC, buds more advanced, with the line of life leading to each bud at DDD. Fig. 4 are three screws of the same without the outward marks of the buds (that it may not confuse), only showing the piece which will draw up or increase: there should be an inch and a half between each in the natural size; from E to E is the lengthening piece. Fig. 5 shows the way the wood increases in the contrary direction, I mean in width. FF is the row of alburnum deposited *last* autumn, and to be completed *this*, which is now done at GG; while the bending up of the bark-vessels at HH allows the sap to deposit a new row of alburnum at II, which is also seen at fig. 6, where the bending of the bark-vessels leaves it free. They are soon straightened at K, by the enlargement of the side of the circle, which the next spring will be made even by another row on the north side. Fig. 7 shows the increase on one side only, when the climate prevents exotics from receiving their spring shoots in width, and this is no very uncommon case. I have many specimens of the kind by me. Fig. 8 shows the manner of forming the circles—when a tree is past eighty, it then marks its lines *between* the others. But I have to get more specimens, which will complete my knowledge in this respect, which is yet *partly* but conjecture, and therefore not wholly to be trusted to. Fig. 9 is the manner in which the wood-vessels draw one out of the other; but as they lengthen, the upper ones soon decrease to the smallest size.

Drawing to illustrate the manner in which trees increase in growth.

II.

Some Horticultural Observations, selected from French Authors. By the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. P. R. S. &c.*[*]

Peaches.

THOUGH the English excel in many branches of horticulture, there are others in which they are materially outdone by the French. Absolute perfection in any branch of an art, so extensive as that of gardening, cannot be obtained by a person, who allows his talents to range over every part of it. This the French knew long ago, and have regulated their practice accordingly. The English have not yet begun this subdivision of skill. Our fruit gardeners, who carry every sort of fruit to market of a good quality, cannot be said to have

Attention to a particular branch of gardening alone leads to perfection.

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[*] Trans. of the Hort. Soc. vol. i. App. p. 4.

brought any one kind to absolute perfection. In *France*, whole villages are employed in the culture each of one single kind of fruit. In consequence of this arrangement, the fruits, under the management of individuals, who for many generations have exerted their whole energies to this one point only, are brought to a degree of perfection, which can never be attained in a garden, where fruits and vegetables of all sorts must be provided by one man, for a large and opulent family, or for a weekly market.

Peaches only cultivated at Montreuil.

At *Montreuil**, a village near *Paris*, the whole population has been maintained, for several generations, by the cultivation of *peaches*, which is their sole occupation. It is there alone, where the true management of this delicious fruit can be studied and attained; for it is impossible, from written precepts, to acquire the whole art. The modes of winter and of summer pruning† are varied not only according to the differences of soil and of exposure, but even according to the state and constitution of each individual tree.

Some of the best raised from the stone.

Some of the best of their fruits are never budded, but always reared from the stone; the rest are budded on stocks of a half wild *peach*, called *peche de vigne*.

Almond the best stock for budding.

Peach trees, budded on an *almond* stock, are larger and more durable than others; but they require a deep and light soil, and do not *fruit* so soon. The best *almonds* for stocks are the red-shelled sort, and some prefer the bitter; but it is more difficult to succeed with these, than with the soft-shelled almond.

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* An English tourist tells us, that he had stored his carriage with *peaches*, which he thought excellent; when he arrived at *Montreuil*, the inhabitants there, who offer their fruit for sale to travellers, told him that he would, if he tasted one of theirs, throw those he had got out of his chaise; which, in fact, he did, as soon as he had tasted a *Montreuil* peach.

Two modes of bearing in fruit-trees.

† Fruit trees may, in respect to their mode of bearing, be divided into annuals or biennials. *Figs*, *walnuts*, &c., are annuals, that is, they bear their fruit on the branches of the present year; *peaches* and *pears*, &c., are biennials, their fruit is produced on wood of the second year's growth. In this case much advantage is derived from the practice of rubbing off the leaf buds of the fruit-bearing branches, leaving only as many as are wanted to produce wood for the succeeding year. This, no doubt, is the *taille d'été* of the French; it does not only leave the remaining wood to grow stronger, and to ripen sooner, but it materially increases the size of the fruit. The French use this method with their *figs*, as is noticed in page 254.

Apricot and plum stocks.

Stocks of the *apricot*, and of the *prune de St. Juliers*, produce smaller trees, that bear sooner, but do not last so long, and, of course, answer better in a shallow soil.

Season of budding.

The season of budding depends on the weather being more or less wet; the end of *July*, in ordinary years, is proper for the *plum* stock, that for the *apricot* and the *almond* stock is later: and for the young *almond* stock, the middle of *September* is the most proper.

Raising stocks.

In order to provide stocks, the fruit stones are sown in baskets; which, when the tree has attained a proper size, are sunk in the ground where it is intended they should grow, provided the soil is deep; for shallow soils the young plant is taken up, and its larger roots cut off, which forces it to throw out lateral roots, and in the event to become a more productive bearer.

The trees in blossom should be protected from frost.

The climate of *France* is certainly better suited to the culture of the *peach*, than that of *England*, as some sorts produce their fruit there in perfection on espaliers, and a few on standards in the open air. The people of *Montreuil* are, however, abundantly more careful, than we are, to protect their trees from the action of frosts, during the time of flowering: at that time a very slight degree of frost is apt to seize upon the pistil; and if the sun shine upon the flower before it is entirely thawed, this organ loses its power of receiving the pollen, and the flower, in consequence, drops off without setting its fruit.

Modes of doing this.

To guard against this, the tops of the peach walls are furnished with long wooden pegs, or with iron wall-hooks, on which planks are fixed; and on them straw mats are hung in such a manner as to be rolled up or let down at pleasure.

Those who do not use this precaution, light fires with damp straw in such a manner, that the smoke may pass over the flowering branches at sun-rise. This intercepts, in some degree, the direct rays of the sun, and, by its gentle warmth, thaws the frozen pistils by gradual and slow degrees; others fasten the branches cut from ever-green trees, with their leaves upon them, in front of the *peach* trees, to break off the cold air.

The fruit should not ripen on the tree.

Peaches are never eaten in perfection if suffered to ripen on the tree; they should be gathered just before they are quite soft, and kept at least twenty-four hours in the fruit chamber.

Figs.

Figs cultivated at Argenteuil.

The inhabitants of *Argenteuil,* near *Paris,* derive their chief support from the culture of *fig* trees; near that town are immense fields covered with these trees, on the sides of hills facing the South, and in other places sheltered from the North and the North-west winds.

The branches buried to protect them from frost.

In the autumn the earth about the roots of these trees is stirred and dug; as soon as the frosts commence, the gardeners bend down the branches, and bury them under six inches of mould, which is sufficient to preserve them from being frozen.

The branches must be entirely stripped of their leaves before this is done; the gardener then, taking hold of the top of each branch, bends it down gradually, and with much care, to prevent its breaking, placing his knee or his hand under such parts as resist the most; the branches that will not bend low enough to be buried are cut off close to the ground.

A fig-tree will remain buried in this manner seventy-five or eighty days without harm; when the season is mild, the gardeners uncover them, especially in times of warm rains, but on the first symptoms of frost they are again buried. Severe frosts sometimes reach them, but the branches only are destroyed. The roots produce a new crop in the summer; but these do not bear fruit till the next year, and are more tender and liable to be killed by frost during the next winter, than older and more woody branches.

Leaf buds pinched out from the fruit buds.

In the spring the trees are carefully inspected; and where a double bud is observed, the gardeners, who are able to distinguish a leaf-bud, which is more sharp, from a fruit-bud, which is rounder, pinch out the leaf-buds without hurting the fruit-buds; these, as they receive the sap prepared by the plant for two purposes, produce fruit of double the ordinary size; this is done at *Paris* between the first and the tenth of *June*; but these leaf-buds may be suffered to expand a little, till they can be distinguished with certainty; they must not be all destroyed at the same time. In cool seasons, the ripening of the fruit is hastened by inserting a drop of oil in the eye, from the point of a pen, or tooth-pick.

Artificial ripening.

It is necessary in dry seasons to water *fig trees*; the nature of the plant requires to have its root cool, while its head is exposed to the hottest sun. If planted against the south wall of a house near a spout that brings water from the roof, it thrives luxuriantly. Figs do well also in a paved court; the stones keep the ground under them moist and cool, while the surrounding buildings reflect and increase the heat of the sun's rays.

In dry seasons they require water.

Good situations.

Apricots.

Our gardeners believe that the *Moor-park apricot* is the fruit called *abricot pêche* by the French; but this is a mistake, the *abricot pêche* is a large tree, which may be raised from the stone without grafting: it ripens later than the rest, not till the end of *August*. The stone is so soft, that a pin will pierce through it: the kernel is bitter.

Apricot.

Pears.

The *crassane* may be improved, and all its harshness destroyed, by grafting upon the *doyenné*, a pear known in our gardens.

Pear.

Apples.

The *golden pippin (reinette d'Angleterre)* is described not only as an excellent fruit, but as a very productive bearer; in *England* it appears to be in its last stage of decay. It is probable that trees decay by age sooner in colder than in warmer climates.

Golden pippin.

The French do not suffer their *apple* or their *pear* trees, to form wild heads as ours do, and shade all things planted near them; their standard trees, of all kinds, when in gardens, are trained in such manner as to cast the least shade possible. A form like a pyramid, called by them *quenouille*, is very generally used.

Shape given to standards.

Plums.

The *green gage*, called in French *la reine Claude*, is much improved, if grafted on an *apricot* or a *peach* stock*.

Green gage.

Maize,

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* The name of *green gage* is said to have originated from the fol-

Etymology.

Maize, Egg Plant, and Sweet Potatoes.

All these plants are reared for use in some kitchen gardens of *France*, though probably not in many.

Indian corn.

Maize is sown in the ground, without heat; when the spike is about half an inch thick, it is eaten fried in butter, as artichokes are, or made into pickle with vinegar.

Egg plant.

The *egg plant* is called in the gardens *la plante qui pond.* The seeds of this, as of the other varieties of solanum, are sown on a hotbed, in March; the plants, when ready, are transplanted into pots, and plunged in a gentle heat; after the plant has advanced considerably, it may be placed in the open air. The fruit is much used for ragouts in Provence.

Sweet potato.

The *sweet potato*[*] is planted on a hotbed in the middle of *April*, in about six inches of mould: when the shoots are eight or ten inches long, they may be taken up, and replanted in a bed of light mould, in the open air, about eighteen inches deep: all the leaves, except the uppermost, are first to be taken off, and the shoot then buried so deep, that the small bunch of leaves only appears above ground.

In *October* the tubers are ripe and ready to be dug up; in doing this, the greatest care must be taken not to wound the skin, as the slightest scratch disposes them to rot.

They must be kept free from frost and damp; if exposed to either of these, they exhale an odour like that of the rose, and rot immediately. Both the yellow and the red variety are cultivated in *France*; the red is preferred.

Strawberries.

Alpine strawberry.

The French cultivate the *alpine strawberry* in the mode recommended by Mr. A. KNIGHT in the Horticultural Transactions[†], and find the fruit so much better when produced by

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lowing accident. The *Gage* family, in the last century, procured from the monks of the *Chartreuse*, at *Paris*, a collection of fruit trees: these arrived at their mansion of *Hengrave Hall*, with the tickets safely affixed to them, except only the *reine Claude*, the ticket of which had been rubbed off in the passage. The gardener being, from this circumstance, ignorant of the name, called it, when it bore fruit, the *green gage*.

[*] Convolvulus *batatas*, *L*.

[†] Journal, vol. xxix, p. 214.

seedlings of the first year, that they seem to prefer the *alpine* to all other sorts, and to be supplied at market with the fruit of it in every month of the year, by the use of some heat in the winter.

Sowing the seed.

The seeds, they say, may be sown either in a little heat, or in the open air, but always in the shade; they should be sown in sifted mould, and scarcely covered; have a thin layer of moss strewed over them, and they should be frequently moistened. Fresh seed grows up in eighteen days: old seed is much slower. The runners must be carefully removed.

The market gardeners near *Paris* sow theirs twice a year, in *March*, and toward the end of *August*; in six weeks they are large enough to be transplanted, which is done at eight inches apart. Those sown in *March*, fruit in *May* and *June*; those sown in *August*, the spring following. See *Traité des Arbres*, p. 9. I rather suppose that the plants sown in *March* give their fruit in autumn.

It is good to sow *strawberries* within the distance of five or six feet from a north or a west wall; in the latter case, the moss is absolutely necessary. The plants grown from the *March* sown seed must be well watered through the summer; in hot weather twice a day, if they are expected to bear in the autumn. The *French* seem to find the *August* sowing most productive. Even in the autumn, in the almanac called *Le bon Jardinier*, the author tells us to sow the seeds of *strawberries* in *February*, if we have not done it in the preceding *August*.

Hautbois and Chili strawberry.

The *hautbois* is called in *French*, *caperonier*; it is lately only that we have observed an hermaphrodite variety, which bears abundantly; in fact, the plant is polygamous: this the *French* have long known, and they say that the *Chili strawberry* is also polygamous, and that the females may be made fertile by the impregnation of the male flowers of the *hautbois*.

III.

Farther Experiments and Observations on the Action of Poisons on the Animal System. By B. C. Brodie, *Esq. F. R. S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society*[*].

Former observations on poisons.

Recovery from apparent death.

Effects of mineral and vegetable poisons differ: medicine may be improved by the investigation.

I. SINCE I had the honour of communicating to the Royal Society some observations on the action of certain poisons on the animal system[†], I have been engaged in the farther prosecution of this inquiry. Beside some additional experiments on vegetable poisons, I have instituted several with a view to explain the effects of some of the more powerful poisons of the mineral kingdom. The former correspond in their results so nearly with those which are already before the public, that, in the present communication, I shall confine myself to those which appear to be of some importance, as they more particularly confirm my former conclusions respecting the recovery of animals apparently dead, where the cause of death operates exclusively on the nervous system. In my experiments on mineral poisons, I have found some circumstances wherein their effects differ from those of vegetable poisons, and of these I shall give a more particular account. Whatever may be the value of the observations themselves, the subject must be allowed to be one that is deserving of investigation, as it does not appear unreasonable to expect, that such investigation may hereafter lead to some improvements in the healing art. This consideration, I should hope, will be regarded as a sufficient apology for my pursuing a mode of inquiry by means of experiments on brute animals, of which we might well question the propriety, if no other purpose were to be answered by it than the gratification of curiosity.

Former account given more at large:

In my former communication on this subject, I entered into a detailed account of the majority of my experiments. This I conceived necessary, because in the outset of the inquiry I had been led to expect, that even the same poison might not always operate precisely in the same manner; but I have since had abundant proof, that in essential circumstances there is but little variety in the effects produced by poisons of any description, when employed on animals of the same, or even of different species, beyond what may be referred to the difference in the quantity, or mode of application of the poison, or of the age and power of the animal. This will explain the reason of my not detailing, in the present communication, so many of the individual experiments from which my conclusions are drawn, as in the former: at the same time I have not been less careful to avoid drawing general conclusions from only a limited number of facts. Should these conclusions prove fewer, and of less importance than might be expected, such defects will, I trust, be regarded with indulgence, at least by those who are aware of the difficulty of conducting a series of physiological experiments; of the time which they necessarily occupy; of the numerous sources of fallacy and failure which exist; and of the laborious attention to the minutest circumstances, which is, in consequence, necessary, in order to avoid being led into errour.

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[*] Phil. Trans. for 1812, p. 205.

[†] Phil. Trans. for 1811, p. 178; or Journal, vol. XXX, p. 295, 324.

but the effects of poisons differ but little.

Hence fewer experiments are detailed.

Difficulty of physiological experiments.

II. *Experiments with the Woorara.*

In a former experiment I succeeded in recovering an animal, which was apparently dead from the influence of the essential oil of bitter almonds, by continuing respiration artificially until the impression of the poison upon the brain had ceased; but a similar experiment on an animal under the influence of the woorara was not attended with the same success. Some circumstances led me to believe, that the result of the experiment with the woorara might have been different if it had been made with certain precautions; but I was unable at that time to repeat it, in consequence of my stock of the poison being exhausted. I have since, however, been able to procure a fresh supply, and I shall relate two experiments which I have made with it. In one of these, an animal apparently dead from the woorara, was made to recover, notwithstanding the functions of the brain appeared to be wholly suspended for a very long period of time; in the other, though ultimate recovery did not take place, the circulation was maintained for several hours after the brain had ceased to perform its office.

Artificial respiration recovered an animal from the poison of bitter almonds; but not from that of woorara:

yet it has finally succeeded with this.

Exp. 1. A cat poisoned with woorara.

Experiment 1. Some woorara was inserted into a wound in a young cat. She became affected by it in a few minutes, and lay in a drowsy and half-sensible state, in which she continued at the end of an hour and fifteen minutes, when the application of the poison was repeated. In four minutes after the second application, respiration entirely ceased, and the animal appeared to be dead; but the heart was still felt acting about one hundred and forty times in a minute. She was placed in a temperature of 85 of Fahrenheit's thermometer, and the lungs were artificially inflated about forty times in a minute.

The heart continued acting regularly.

When the artificial respiration had been kept up for forty minutes, the pupils of the eyes were observed to contract and dilate on the increase or diminution of light; saliva had flowed from the mouth, and a small quantity of tears was collected between the eye and eyelids; but the animal continued perfectly motionless and insensible.

At the end of an hour and forty minutes, from the same period, there were slight involuntary contractions of the muscles, and every now and then there was an effort to breathe. The involuntary motions continued, and the efforts to breathe became more frequent. At the end of another hour the animal, for the first time, gave some signs of sensibility when roused, and made spontaneous efforts to breathe twenty-two times in a minute. The artificial respiration was discontinued. She lay, as if in a state of profound sleep, for forty minutes, when she suddenly awoke, and walked away. On the following day she appeared slightly indisposed; but she gradually recovered, and is at this time still alive and in health.

Exp. 2. A rabbit.

Experiment 2. Some woorara was applied to a wound in a rabbit. The animal was apparently dead in four minutes after the application of the poison; but the heart continued acting. He was placed in a temperature of 90°, and the lungs were artificially inflated. The heart continued to act about one hundred and fifty times in a minute. For more than three hours the pulse was strong and regular; after this it became feeble and irregular, and at the end of another hour the circulation had entirely ceased. During this time there was no appearance of returning sensibility.

The circulation of the blood may be maintained in an animal from

The circulation cannot be kept up above a certain time without the brain.

from which the brain has been removed for a considerable, but not for an unlimited, time. We may conclude, that in the last of these experiments the animal did not recover, because the influence of the poison continued beyond the time during which the circulation may be maintained without the brain.

III. *On the Effects of Arsenic.*

Two opinions of the effect of arsenic.

Death is not the result of inflammation of the stomach.

When an animal is killed by arsenic taken internally, the stomach is found bearing marks of inflammation; and it is a very general opinion, 1, that this inflammation is the cause of death: 2, that it is the consequence of the actual contact of the arsenic with the internal coat of the stomach. But in several cases I have found the inflammation of the stomach so slight, that on a superficial examination it might have been easily overlooked; and in most of my experiments with this poison death has taken place in too short a time for it to be considered as the result of inflammation: and hence we may conclude, that the first of these opinions is incorrect; at least as a general proposition.

The inflammation that exists not caused by the contact of the arsenic.

Many circumstances conspire to show, that the second of these opinions also is unfounded.

In whatever way the poison is administered, the inflammation is confined to the stomach and intestines—I have never seen any appearance of it in the pharynx or œsophagus.

If applied to a wound, the stomach becomes inflamed.

Mr. Home informed me, that in an experiment made by Mr. Hunter and himself, in which arsenic was applied to a wound in a dog, the animal died in twenty-four hours, and the stomach was found to be considerably inflamed.

I repeated this experiment several times, taking the precaution always of applying a bandage, to prevent the animal licking the wound. The result was, that the inflammation of the stomach was commonly more violent and more immediate, than when the poison was administered internally, and that it preceded any appearance of inflammation of the wound*.

Some

Dr. Jaeger of the same opinion with the author.

* Since the greater part of my experiments on this subject were made, I have seen an account of an inaugural dissertation on the effects of arsenic, by Dr. Jaeger of Stuttgard. Dr. Jaeger has come to conclusions similar to those above stated, that in an animal killed by arsenic, the inflammation of the stomach is not the cause of death, and that the

poison

Vegetable poisons act on the system through the blood:

so does arsenic.

Some experiments are already before the public, which led me to conclude, that vegetable poisons, when applied to wounded surfaces, affect the system by passing into the circulation through the divided veins. From this analogy, and from all the circumstances just mentioned, it may be inferred, that arsenic, in whatever way it is administered, does not produce its effects even on the stomach until it is carried into the blood.

How far the blood is necessary to life.

But the blood is not necessary to life, except so far as a constant supply of it is necessary for the maintenance of the functions of the vital organs. The next object of inquiry therefore is, when arsenic has entered the circulation, on what organs does it operate, so as to occasion death?

Arsenic causes a slough, not as a caustic, but by killing the part.

Acting as a poison, it does not destroy the life of every part at once.

When arsenic is applied to an ulcerated surface, it produces a slough, not by acting chemically, like caustics in general, but by destroying the vitality of the part to which it is applied, independently of chemical action. This led me at first to suppose, that, when arsenic has passed into the circulation, death is the consequence, not so much of the poison disturbing the functions of any particular organ, as of its destroying at once the vitality of every part of the system. The following circumstances, however, seem to show, that this opinion is erroneous. In an animal under the full influence of arsenic, even to the instant of death, some of the secretions, as those of the kidneys, stomach, and intestines, continue to take place in large quantity; and the muscles are capable of being excited, after death, to distinct and powerful contractions by means of the Voltaic battery.

Exp. 3. Arsenic applied to a wound in a rabbit.

Experiment 3. Seven grains of the white oxide of arsenic were applied to a wound in the back of a rabbit.

In a few minutes he was languid, and the respirations were small and frequent. The pulse was feeble, and after a little time could not be felt. The hind legs became paralysed[*]. He

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poison does not produce its fatal effects until it has entered the circulation. I have to regret, that I have had no opportunity of seeing the original of this dissertation.

The influence of the brain less easily con-

[*] I have observed, that, where the functions of the brain are disturbed, paralysis first takes place in the muscles of the hind legs; afterward in those of the trunk and fore legs; and last of all in the muscles of the ears

He grew insensible, and lay motionless, but with occasional convulsions. At the end of fifty-three minutes from the time of the arsenic being applied, he was apparently dead; but on opening the thorax, the heart was found still acting, though very slowly and feebly. A tube was introduced into the trachea, and the lungs were artificially inflated; but this appeared to have no effect in prolonging the heart's action. On dissection, the inner membrane of the stomach was found slightly inflamed.

Exp. 4. Arsenic injected into the stomach of a dog.

Experiment 4. Two drams of arsenic acid dissolved in six ounces of water were injected into the stomach of a dog, by means of a tube of elastic gum, passed down the œsophagus. In three minutes he vomited a small quantity of mucus, and this occurred again several times. The pulse became less frequent, and occasionally intermitted. At the end of thirty-five minutes the hind legs were paralysed, and he lay in a half sensible state. At the end of forty-five minutes he was less sensible; the pupils of the eyes were dilated; the pulse had fallen from 140 to 70 in a minute, and the intermissions were frequent. After this, he became quite insensible; convulsions took place, and at the end of fifty minutes, from the beginning of the experiment, he died. On opening the thorax, immediately after death, tremulous contractions of the heart were observed; but not sufficient to maintain the circulation. The stomach and intestines contained a large quantity of mucous fluid, and their internal membrane was highly inflamed.

The experiments repeated with similar results.

These experiments were repeated, and the results, in all essential circumstances, were the same. The symptoms produced were, 1, paralysis of the hind legs, and afterwards of the other parts of the body; convulsions; dilatation of the pupils of the eyes; insensibility; all of which indicate disturbance of the functions of the brain: 2, a feeble, slow, intermitting pulse, indicating disturbance of the functions of the heart. Where the heart has continued to act after apparent

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ears and face. These facts seem to show, that the influence of the brain, like that of the heart, is not propagated with the same facility to the distant as to the near organs; and this is farther confirmed by cases of disease which occasionally occur, in which, although the paralysis is confined to the lower half of the body, the morbid appearances met with on dissection are entirely confined to the brain.

veyed to remote parts.

death, I have never, in any one instance, been able to prolong its action by means of artificial respiration. 3, pain in the region of the abdomen; preternatural secretion of mucus from the alimentary canal; sickness and vomiting in those animals which are capable of vomiting; symptoms which arise from the action of the poison on the stomach and intestines. There is no difference in the effects of arsenic, whether it is employed in the form of white oxide, or of arsenic acid, except that the latter is a more active preparation. When arsenic is applied to a wound, the symptoms take place sooner than when it is given internally; but their nature is the same.

Arsenic applied to wounds acts most speedily.

Effects of arsenic.

The symptoms produced by arsenic may be referred to the influence of the poison on the nervous system, the heart*, and the alimentary canal. As of these the two former only are concerned in those functions which are directly necessary to life, and as the alimentary canal is often affected only in a slight degree, we must consider the affection of the heart and nervous system as being the immediate cause of death.

In every experiment which I have made with arsenic, there were evident marks of the influence of the poison on all the organs which have been mentioned; but they were not in all cases affected in the same relative degree. In the dog, the affection of the heart appeared to predominate over that of the brain, and on examining the thorax immediately after death, this organ was found to have ceased acting, and in a distended state. In the rabbit, the affection of the brain appeared to predominate over that of the heart, and the latter was usually

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The heart in some respect independent of general affection of the nervous system.

* When I say, that a poison acts on the heart, I do not mean to imply, that it necessarily must act directly on the muscular fibres of that organ. It is highly probable, that the heart is affected only through the medium of its nerves; but the affection of the heart is so far independent of the affection of the nervous system generally, that the circulation may cease although the functions of the brain are not suspended, and the functions of the brain may be wholly suspended without the circulation being at all disturbed. In proof of the first of these propositions, I may refer to my former experiments on the upas antiar, in which the sensibility of the animal continued to the very instant of death; and respiration, which is under the influence of the brain, continued even after the heart had ceased to act. In proof of the second, I may refer, among many others, to the experiments detailed in the Croonian Lecture for 1810.—[Phil. Trans. for 1811, pp. 36, 211; or Journal, vol. XXIX, p. 359.]

found acting slowly and feebly, after the functions of the brain had entirely ceased. In the rabbit, the effects of the arsenic on the stomach and intestines were usually less than in carnivorous animals.

Action of arsenic less simple than that of most vegetable poisons.

The action of arsenic on the system is less simple than that of the majority of vegetable poisons. As it acts on different organs, it occasions different orders of symptoms; and as the affection of one or another organ predominates, so there is some variety in the symptoms produced even in individual animals of the same species.

Appearances after death.

In animals killed by arsenic the blood is usually found fluid in the heart and vessels after death; but otherwise all the morbid appearances met with on dissection are confined to the stomach and intestines. As this is the case, and as the affection of these organs occasions remarkable symptoms, it may be right to mention the result of my observations on this subject.

State of the alimentary canal.

In many cases where death takes place, there is only a very slight degree of inflammation of the alimentary canal: in other cases the inflammation is considerable. It generally begins very soon after the poison is administered, and appears greater or less, according to the time which elapses before the animal dies. Under the same circumstances, it is less in graminivorous, than in carnivorous animals. The inflammation is greatest in the stomach and intestines; but it usually extends also over the whole intestine. I have never observed inflammation of the œsophagus. The inflammation is greater in degree, and more speedy in taking place, when arsenic is applied to a wound, than when it is taken into the stomach. The inflamed parts are in general universally red, at other times they are red only in spots. The principal vessels leading to the stomach and intestines are turgid with blood; but the inflammation is usually confined to the mucous membrane of these viscera, which assumes a florid red colour, becomes soft and pulpy, and is separable without much difficulty from the cellular coat, which has its natural appearance. In some instances there are small spots of extravasated blood on the inner surface of the mucous membrane, or between it and the cellular coat, and this occurs independently of vomiting. I have never, in any of my experiments, found ulceration or sloughing of the stomach or intestine; but if the animal survives for a certain length of time after the inflammation has begun, it is reasonable to conclude, that it may terminate in one or other of these ways,

Sloughing seldom, if ever, the direct consequence of the application of arsenic to the stomach or intestines.

I am disposed to believe, that sloughing is very seldom, if ever, the direct consequence of the application of arsenic to the stomach or intestines. Arsenic applied to an ulcer will occasion a slough; but its action in doing this is very slow. When I have applied the white oxide of arsenic to a wound, though the animal has sometimes lived three or four hours afterward, and though violent inflammation has taken place in the stomach and intestines, I have never seen any preternatural appearance in the part to which it was applied, except a slight effusion of serum into the cellular membrane. Arsenic speedily produces a very copious secretion of mucus and watery fluid from the stomach and intestines, which separates it from actual contact with the inner surface of these organs, even though taken in large quantity and in substance; and in animals which are capable of vomiting, by much the greater part is rejected from the stomach very soon after it has been taken in. Hence though a few particles of arsenic are sometimes found entangled in the mucus, or in the coagulum of extravasated blood, and adhering to the inner surface of the stomach, I have never seen it in such a quantity as might be supposed capable of producing a slough.

Supposed sloughs from its action on the stomach.

In one instance, where a dog had swallowed a large quantity of arsenic in substance, a brown spot, about an inch in diameter, was observed after death on the inner surface of the cardiac extremity of the stomach, having so much of the appearance of a slough that at first I had no doubt of its being so; but on examination this proved to be only a thin layer of dark-coloured coagulum of blood, adhering very firmly to the surface of the mucous membrane, and having a few particles of arsenic entangled in it. On removing this the mucous membrane still appeared of a dark colour; but this was also found to arise from a thin layer of coagulum of blood between it and the cellular coat. The mucous membrane itself was inflamed; but otherwise in a natural state. I have observed a similar appearance, but occupying a less extent of surface, several times. In the Hunterian Museum there is a human stomach, which was preserved to show what was considered as a slough produced by the action of arsenic. On examining this preparation, I found that the dark-coloured spot, which had been supposed to be a slough, was precisely of the same nature with that just described.

Although the affection of the stomach and intestines from arsenic is not the cause of death under ordinary circumstances, it is reasonable to conclude, that it may be so in some instances, if the animal survives the effects produced on the organs more immediately necessary to life. Mr. Henry Earle informed me of an instance, in which this appeared to be the case. A woman in St. Bartholomew's hospital, who had taken arsenic, recovered of the immediate symptoms, but died at the end of four or five days. On examination after death, extensive ulcerations were found of the mucous membrane of the stomach and intestines, which we can hardly doubt to have been the cause of death.

The affection of the stomach and intestines from arsenic sometimes kills ultimately.

It is an important matter of inquiry, as connected with judicial medicine, how far may the examination of the body, after death, enable us to decide, whether an animal has died of the effects of arsenic? On this subject, however, I have only a few remarks to make.

Important judicial inquiry. How far is it possible to ascertain poisoning by arsenic from examination of the dead body?

The inflammation from arsenic, occupying in general the whole of the stomach and intestine, is more extensive than that from any other poison with which I am acquainted. It does not affect the pharynx or œsophagus, and this circumstance distinguishes it from the inflammation which is occasioned by the actual contact of irritating applications.

But little in general is to be learnt from the examination of the contents of the stomach after death. When arsenic has been taken in substance, small particles of it are frequently found entangled in the mucus, or in the extravasated blood; but where this was not the case, I have never known, in an animal that was capable of vomiting, that arsenic could be detected in the contents of the stomach after death, though examined by the most accurate chemical tests. As some substances when taken internally are separated from the blood very soon afterward with the urine, I thought it probable, that arsenic might be separated with the urine also; but Mr. Brande (to whom I am indebted for assistance on this, as well as on

many

[illegible] could never detect the smallest trace of [illegible].

(To be concluded in our next.)

[illegible]

Description of an Apparatus by means of which all bad Smell may be avoided in manufacturing Prussian Blue: by Mr. D'ARCET.*

[illegible] manufactories of Prussian blue diffuse to a distance [illegible] smell. The first, that produced from the [illegible] matter, is easily avoided by covering the [illegible] with a [illegible], at the summit of which is the chimney [illegible] setting fire to the vapours emitted from the [illegible] as soon as they are hot enough to burn.

The second source of the bad smell is found in the use of [illegible] alum, which contains more or less sulphate of iron. When the mixture of blood and potash is calcined, the temperature is high enough for the sulphate to be decomposed and converted into sulphuret, by means of the animal charcoal [illegible]: whence it follows, that the Prussic [illegible] hydrosulphuret of potash in solution; and that, when the liquor is mixed with the solution of alum and sulphate of iron, a large quantity of sulphuretted hidrogen [illegible], which is extremely fetid, and diffuses itself [illegible] spoiling meat, that is within [illegible].

By means of the apparatus about to be described, these inconveniences may be avoided, and the sulphuretted hidrogen [illegible] the mixture of the two liquids may even be [illegible].

[illegible] is a tub of white wood, well hooped, and [illegible] pieces of wood, that raise it from the ground and preserve the bottom from rotting.

[illegible] a hemisphere of thin copper, of the same diameter as the tub, and serving as a cover to it. It fits into it up to the rim that appears in the figure. Before the cover is put on, the edge of the tub and the lower face of the rim are to be coated with potter's clay well diluted, which will render the juncture perfect.

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* Ann. de Chim. vol. LXXXII, p. 165.

c is a copper tubulure, through which the stem of the beater *h* is to be passed, before the cover is put on the tub.

h elevation of the beater. Toward the upper end of the stem is seen a piece of skin fastened to it. When the beater is placed in the tub, and the stem passes out through the cover, the lower part of the skin is fastened to the rim of the tubulure, and thus the communication with the air is prevented, without prejudice to the movement of the beater. The skin used should be soaked in oil, that it may not be injured by the liquids put into the tub.

g plan of the foot of the beater.

d funnel through which the different solutions are poured into the tub.

l a wooden plug used to stop the neck of the funnel.

i a cock, or spigot and faucet, by which the Prussian blue is drawn off from the tub, after the solutions have been well mixed in it.

n a small tub, sunk into the ground, into which the result of the mixture runs. As the liquid Prussian blue runs into this, it is dipped out with a ladle into a bucket, to be carried to the casks, in which it is to be washed with a large quantity of water.

e a curved tube fixed to the dome.

f a tube of the same diameter fixed in the ground. The dotted lines, terminating at *m*, point out the situation of this tube, which is placed parallel with the surface of the ground, and terminates in the ash-pit, near the grate of the furnace where the prussiate of potash is prepared. When the cover is put down on the tub, the tube *e* should enter into the tube *f*, and the juncture is to be luted with a little potter's clay.

Mode of using it.

Fig. 2 represents the apparatus put together, and ready for use. When the solutions are prepared, the door of the ash-pit, in which the tube terminates, is to be shut close; the plug *l* is to be taken out of the funnel, and the solution of alum and sulphate of iron poured in. A workman mounts a little stool, takes hold of the stem of the beater *h*, and begins to agitate the liquor in the tub. Two others pour the prussic liquor gently into the funnel *d*, and the workman who holds the beater moves it in all directions, that the liquors may be intimately mixed. A little of the mixture is drawn off occasionally by the cock *i*, filtered through blotting paper, and examined, to find whether a sufficient quantity of prussiate of potash have been poured in: if not, more is added; and, when it has reached the point of saturation, no farther addition is to be made, but the stirring of the mixture with the beater is to be continued about ten minutes.

The ash-pit of the furnace being closed, the draught of the fire causes the outer air to enter through the tube of the funnel *d*; this air mixes with the gasses extricated from the mixture, and the whole is conveyed through the tube *e m* underneath the grate of the furnace, where the sulphuretted hidrogen takes fire, loses thus its offensive smell, and serves also to keep up the heat of the crucible.

When the stirring of the mixture is finished, the tub may be emptied by the cock, and a new mixture immediately commenced.

The cover of the tub need not be taken off, except when the apparatus wants repair. When it is left some time out of use, the tub should be kept full of water, and this water may be employed afterward for lixiviating the residuum of the calcination of the blood and potash.

It has been employed with success.

The apparatus here described and figured I have caused to be fitted up at the paper-hanging manufactory of the brothers Jacquemart. It has succeeded completely, no inconvenience has been found in its use, and it has entirely freed the work-shops and neighbourhood from the bad smell diffused by mixing the prussic liquor, and the solution of alum and sulphate of iron.

Note of the French Editors.

A similar apparatus at another manufactory.

An apparatus of this kind, but by no means so well constructed, has also been employed with success for several years in a manufactory of Prussian blue in St. Nicholas-street.

V.

Extract from a Letter addressed to Mr. D'ARCET *by Mr.* DUFAUD, *Director of the Iron Works at Montalaire, near Creil**.

SIR,

I HAVE undertaken, with the greatest pleasure, the experiments on sawing hot cast iron, that you recommended to me: I have followed your instructions; my trials have been attended with the most complete success, and I hasten to give you an account of them.

Sawing heated cast iron suggested.

These experiments were the more interesting to me, as I have since applied them to practical purposes.

My first trial was made with the support of a grate, 108 mil. [4·25 in.] thick. This piece of cast iron was heated in a forge fire with coal: and as soon as it had acquired a sufficient degree of *incandescence* [this is the French term] it was placed on an anvil, and I sawed it with a common carpenter's saw, without any difficulty, and without any injury to the saw, which I dipped immediately into cold water. The carpenter continued to work with the same saw, without having any occasion to repair it.

Experiment.

In this my first trial a little accident occurred. The end of the iron I was sawing off not being supported, it broke, when 20 or 25 m. (about a line) remained to be cut through; but this slight defect I immediately removed with the saw. Convinced of the ease with which a common saw would cut hot cast iron, I afterward applied it to the demands of the ironworks.

The iron broke, as wood would have done, for want of support at the end.

I had occasion to shorten a pivot of 135 m. [5·3 in.] in diameter; but, afraid of its breaking if I cut it cold, an operation besides very tedious and uncertain, unless executed in a lathe, I had resolved to cast another, when the experiment I have just mentioned determined me to saw it.

Second experiment.

Having marked the place of section with red lead, I placed the pivot in a reverberatory furnace; and when I thought it sufficiently hot, I had it taken out of the furnace, and placed on an iron support, so that the two ends had equal bearings.

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* Ann. de Chim. vol. LXXXII, p. 218.

In four minutes, with two saws, which I used and cooled alternately, the piece was cut off, to the great astonishment of my workmen, who found the saws unhurt.

Third experiment.

The same day I performed a still more difficult operation. I had an anvil, which I was about to cast afresh, because it was 41 m. [1·6 in.] too thick, so that it could not be placed in its bed.

I marked the place of the saw kerf with red lead. The two cuts to be made were 217 m. [8·5 in.] long, by 189 m. [7·4 in.] high; and the thinness of the piece to be cut off required precision. This anvil was heated in a reverberatory furnace, in the same manner as the pivot; and, when sufficiently hot, two workmen took hold of it with a strong pair of tongs, and laid it on a block of cast iron. It was cut with much ease and precision by the same saws that had been used in the preceding instance.

General remarks.

In the course of these experiments I remarked,

1, That hot cast iron may be sawed as easily, and in the same space of time, as dry wood.

2, That, to diminish the resistance, the saw should be set fine.

3, That iron heated in a furnace saws more easily than if heated in a forge: and the reason is simple; in a furnace it is heated equally throughout, while in a forge the part near the tewel is almost in a state of fusion, while that opposite to it is scarcely red-hot.

4, That the iron must not be made too hot; for, if its surface be too near a state of fusion, the saw will be clogged, and the process will not go on well.

5, That the saw should be moved very quickly, because then it will be less heated, make its way better, and the cut will be more clean and exact.

6, Lastly, that the iron should be so placed as to have a firm bearing every where, except where the saw is to pass, otherwise it is liable to break before the cutting is finished.

These, Sir, are the whole of my experiments and observations; and I shall be well pleased if they answer your views.

The practice may be of ex-

It is the more to be wished, that this method of cutting cast iron should be rendered as public as possible, as it may be happily applied in many arts. I thank you much for having suggested it to me, for I shall find frequent occasions for it.

tensive use.

Note by Mr. D'ARCET.

Several years ago Mr. Pictet observed a workman saw a cast iron pipe in the workshop of Mr. Paul, of Geneva. He had lately occasion to mention this to Mr. Thenard, who afterward communicated it to Mr. Mollard. Mr. Mollard, struck with the uses to which it might be applied, tried it at the Conservatory of Arts and Trades on pieces of cast iron 7 cent. [2·75 in.] square, and on plates of different thicknesses.

Practised some years ago at Geneva.

Experiments at Paris.

Mr. Mollard used a common saw, and succeeded perfectly with these various pieces, without injuring its teeth. He observed, that the iron should be heated only to a cherry red; and that it should be cut briskly, using the whole length of the saw. Mr. Mollard afterward found, that this process was known to a workman of Mr. Voyenne, who practised it in fitting the cast iron plates used for making stoves. It is probable, that this simple operation may be known in other workshops; but it is lost, as it were, since persons of distinction in the arts are generally ignorant of it.

Instructions.

Known to others.

We see, that the experiments mentioned in Mr. Dufaud's letter confirm the account of Mr. Pictet, and the trials of Mr. Mollard: of course there remains no doubt of the possibility of cutting cast iron when hot, or of the utility of the process.

The practicability demonstrated.

We conceive it would be practicable to employ it in the fabrication of iron cannons, for cutting off the cap of the piece, and even for removing the square piece left at the extremity of the button, which serves for mounting it on the boring machine. Perhaps advantage might be taken of the red heat, which the cannon retains long after it is cast, for sawing off the cap in the mould itself, its upper part only being removed.

Uses to which it may be applied: in making cannon,

The same process would certainly furnish an easy and ready method of cutting a cannon to pieces, and thus rendering it unserviceable; or facilitate its melting in the reverberatory furnace, when required to be cast afresh. Perhaps it might be employed also to ascertain the different ranges of a piece of cannon, shortened by little and little. It seems to us, the knowledge of a practice applicable to so many purposes of the arts cannot be too generally made known.

destroying or remelting them,

and ascertaining their ranges.

VI.

On the liquid Sugar of Starch, and the transmutation of sweet Substances into fermentable Sugar : by Mr. Vogel. *Abridged by* Mr. Bouillon-Lagrange[*].

Sugar had not been chemically compounded.

No chemist has hitherto been able to form sugar by chemical agents. It is true, that Fourcroy and some others supposed, that at some time or other we should perhaps effect the conversion of starch into sugar, as the component parts of these two substances come infinitely near each other.

Supposition that it might be formed from starch,

" Starch," says Fourcroy, " announces itself as a little less carbonated than gum : we may say, that it comes very near to saccharine matter ; and we shall see hereafter, that it appears in fact capable of forming it by a particular alteration of its own substance[†]."

and from gum.

Under the head *gum*, the same chemist expresses himself as follows. " It is not improbable, that art may effect the conversion of gums into saccharine matter ; and already I have several times remarked, that an aqueous solution of gum, through which oximuriatic gas is passed, acquires a saccharine taste, mixed with a strong bitterness. This view of the subject, at present quite novel, will lead to many researches, and to useful results."

Said to have been done :

It is even pretended, that several authors say they have effected this transmutation of fecula into saccharine matter : but how is it possible, that they should have succeeded, and been silent on a fact of such importance ?

but by no one before Kirchoff.

On looking over what has been published by natural philosophers, it appears incontestable, that it was reserved for Mr. Kirchoff, of the imperial academy of Petersburgh, to convert starch into gummy matter, and this into saccharine matter[‡].

His

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[*] Ann. de Chim. vol. LXXXII, p. 148.

Component parts of starch.

[†] According to Messrs. Gay-Lussac and Thenard, starch is composed of

Carbon	43·55
Oxigen	49·68
Hidrogen	6·77
	10·000

Starch ren-

[‡] Mr. Bouillon Lagrange has already found means of renderin

His discovery pursued by Vogel.

His discovery, which opens a new career to vegetable analysis, and may lead to interesting results, has induced Mr. Vogel to pursue these new facts. His first experiments, some particulars of which he has given in the Journal de Physique, differ scarcely in any thing from those of Mr. Kirchoff, except in his observing, that part of the saccharine matter is formed in the course of two hours boiling, and that the proportion of two hundredths of sulphuric acid produces more than that of one hundredth, the quantity mentioned by the chemist of Petersburgh.

Since that time Mr. Vogel has followed up his experiments with more care, in order to acquire an intimate knowledge of the saccharine matter, and the mode of its formation.

Not ready formed in the starch.

To remove every idea of the saccharine matter being the result of simple extraction; a matter that, having escaped fermentation, was concealed by the starch; he washed the starch with a stream of cold water, before he made use of it.

Method of producing it.

When well dried and reduced to powder, he mixed 2 kil. [4lbs. 6¼ oz. avoird.] with 8 kil. of Seine water, acidulated with 40 gr. [0·02 of the weight of the starch] of sulphuric acid at 56° [1·631].

He then boiled the mixture in a silver basin for thirty-six hours. There is no danger of its burning, except during the first hour, when it must be kept constantly stirring with a broad wooden spatula. After that time the mixture grows much more fluid, and requires only to be stirred occasionally.

It is essential to keep up the quantity of water, by adding fresh as it evaporates.

Clarification.

After this boiling, it is to be clarified when cold by means of charcoal and chalk, and the whole is to be filtered through flannel.

Evaporation.

The liquid having been evaporated nearly to a sirupy consistence, it must be left to cool, that more of the sulphate of lime may fall down; after which the clear liquid is to be decanted off, and the evaporation finished.

A silver vessel preferable to tinned copper.

The sugar thus obtained with two hundredths of sulphuric acid in a silver basin was much more saccharine, and less high coloured, than that made in a basin of tinned copper.

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starch soluble in cold water by a slight torrefaction, and thus assimilating it to mucilages. See *Bulletin de Pharm., tom. iii, p. 395.*

dered soluble in cold water.

Lead may be used.

In general the latter cannot be used for the purpose, the tin being strongly attacked by the long continued boiling. A leaden vessel has been substituted for it with success.

Quantity of sugar produced.

The 2 kil. boiled with two hundredths of sulphuric acid yielded, in several comparative experiments, sometimes a little less, sometimes a little more than 2 kil. of sirup at 33° of the areometer [1·295]; so from a mean of them we may conclude, without any material errour, that starch yields its own weight of sirup*.

Many sweet substances contain no sugar.

As many substances have a decidedly sweet taste, for instance sugar of milk, the sweet matter in liquorice, the sweet principle of Scheele (formed during the action of fat oils on litharge in making plasters), without however, containing an atom of sugar, Mr. Vogel thought it necessary to ascertain, in the first place, whether the sweet liquor from starch contained real sugar.

Sugar from starch fermented,

For this purpose he mixed some yeast with 200 gr. [3089 grs.] of sirup of starch in warm water, and put the whole into a phial, communicating with the pneumatic apparatus, by means of a sigmoid tube.

giving out carbonic acid,

Fermentation soon took place, with a very brisk extrication of carbonic acid gas.

and yielding alcohol.

The 200 gr. of sirup yielded by the fermentation upwards of 5 lit. [near 6 quarts] of carbonic acid gas; and a notable quantity of alcohol was obtained by distillation.

Sirup of starch contains gum.

It is certain, that all sirup of starch contains more or less gum, the quantity of which varies extremely, according to the time of boiling, and the weight of the acid employed.

Result of its evaporation.

The most saccharine sirup evaporated slowly in a stove, and dried in tin moulds, afforded a perfectly transparent elastic substance, in every respect similar to the paste of jujubes.

May be employed in pharmacy; but it is deliquescent.

The author has no doubt, that apothecaries may avail themselves of the sirup of starch, for all this kind of gummy saccharine medicaments, particularly those that may remain in a soft state; for the sirup of starch, thus reduced to a solid state, attracts moisture from the air.

Fecula of potatoes equally yields sugar.

Mr. Vogel substituted the fecula of potatoes for starch, and equally obtained a very saccharine gummy sirup.

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* Starch boiled eight hours with four hundredths of sulphuric acid yielded the same results.

The gum was separated by boiling the sirup in a close vessel with alcohol at 30° [0·868.]

The matter on which the alcohol had no action, and which was found in the most perfect sirup to the quantity of two tenths, was very viscous. Being dried and powdered, it exhibited all the characters of gum arabic, namely, its solubility in cold water, forming a thick mucilage, insoluble in alcohol.

The gum similar to gum arabic,

The only character, that appears to distinguish this matter from gum arabic, is its not forming mucous acid with nitric acid.

but does not yield mucous acid.

It has been asserted, however, that the gummy matter precipitated from sirup of starch is a compound of starch, water, and sulphuric acid.

The gum said to be a compound,

To satisfy himself on this head, Mr. Vogel poured a small portion of alcohol into sirup of starch. The precipitate first formed was composed of sulphate of lime and gum. When this was separated, he poured more alcohol into the sirup that had been decanted from it. The second precipitate was gummy matter, unmixed with sulphate: its solution in water was no longer rendered turbid by muriate of barytes.

but this disproved.

The author, however, was not content with this experiment; for it might be objected to him, that the sulphuric acid, being chemically combined with the gum, would not quit it to unite with the barytes. He dissolved this gum therefore in barytes water evaporated to dryness, and gave the mass a strong red heat in a platina crucible: thus the sulphric acid shonld have been set free, and no doubt would have seized on the barytes. Besides, this sulphate would have been decomposed by the carbon of the gum, and converted into a sulphuret: but muriatic acid poured on the calcined matter extricated nothing but carbonic acid gas, and not an atom of sulphuretted hidrogen gas that could be rendered sensible by paper impregnated with acetate of lead.

Farther confirmation of its containing no sulphuric acid.

Besides, the gum distilled on an open fire did not give out any sulphurous acid, or sulphuretted hidrogen gas.

It is not therefore a hydrate of starch combined with sulphuric acid; which affords us a fresh proof, that we must take care not to frame hypotheses before we consult experiment.

Hypotheses not to be hastily formed.

He made the same trials with the sirup deprived of gum by alcohol, which did not precipitate the muriate of barytes; but he

No sulphuric

acid in the sirup.

he could not discover in it the least trace of combined sulphuric acid.

Action of acids on other substances tried.

These experiments could not fail gradually to lead to an examination of the action of acids diluted with water on some other substances. Sugar of milk first drew his attention; and with the greater reason, as we have already announced this substance to become more soluble in water after it has been treated with acid*.

Sugar of milk treated with sulphuric acid.

Mr. Vogel boiled 100 gr. [1545 grs.] of sugar of milk with 400 gr. of water, and 2 gr. of sulphuric acid at 56° [1·631], for three hours, adding more water as it evaporated. After having saturated the excess of acid by carbonate of lime, he filtered.

The liquid, though clear, was slightly coloured. Evaporated slowly in a stove, a thick brownish sirup remained, which concreted into a crystalline mass at the expiration of a few days.

Saccharine product.

This matter resembling soft sugar has a much more saccharine taste than the most concentrated aqueous solution of sugar of milk. From this extremely saccharine taste the author was led to suspect, that a real sugar had been formed, capable of giving rise to the alcoholic fermentation.

Fermented,

In fact this product mixed with yeast diluted with water was scarcely placed in favourable circumstances for the alcoholic fermentation, before it commenced in a very brisk manner; though sugar of milk never ferments, as is well known to all chemists, and has been recently placed beyond all doubt by the numerous experiments of Mr. Bucholz†.

and yielded alcohol.

This fermented liquor yielded a considerable quantity of alcohol. On varying the proportions of sulphuric acid to three, four, and even five hundredths, very saccharine crystals, that ran into fermentation with extreme facility, were constantly obtained, particularly with five hundredths of acid.

Nitric acid has not the same effect.

With two or with four hundredths of nitric acid the sugar of milk could not be converted into a fermentable sugar.

Muriatic acid has.

Acetic acid has not.

Three grammes [46·3 grs.] of muriatic acid converted the sugar of milk into a very saccharine sirup capable of the alcoholic fermentation; while 2 gr. [30·89 grs.] of radical vinegar made no alteration in the sugar of milk.

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[*] See Delamétherie's *Journal de Physique*, July, 1811.

[†] See Delamétherie's Journ. de Physique, for December, 1811.

All these sirups reduced to the crystalline state differ from sugar of milk, not only in being susceptible of the alcoholic fermentation, but also in being very soluble in alcohol, a property that sugar of milk does not possess. Evaporated to dryness by a gentle fire, a white, granular, and extremely saccharine mass is the result.

The substance crystallized differs from sugar of milk.

It remains to explain the manner in which sulphuric acid acts on starch and sugar of milk, to take from them the principle that masks the saccharine substance, or to convert them into fermentable saccharine matters. The author confesses, that it is difficult, and out of his power, to give a clear and plausible theory of this metamorphosis; and, if he risk some notions on this subject, it will be with much reserve.

Theory difficult.

Many are disposed to adopt the opinion, that sugar exists ready formed in starch, and that the sulphuric acid only dissolves or destroys the principle that holds it enchained.

Supposition, that the sugar was formed in the starch.

It is obvious, that this reasoning is in a considerable degree vague; and besides, that it is founded on no experiment, direct or indirect. In this hypothesis too we must imagine a compound altogether new, sugar combined with a substance that renders it insoluble in cold water; and sugar has never yet presented us with such a compound.

Objections.

Others have supposed, that heat alone is capable of effecting this conversion of fecula into saccharine matter; a fact which, if it were confirmed, might throw fresh light on the saccharine fermentation of Fourcroy.

Supposition, that the conversion is effected by heat alone.

Accordingly starch has been boiled with water four days in succession, till it became extremely fluid. The filtered liquor was evaporated, and the result was a thick mucilage, very bitter, without the least taste of sugar. The starch remaining on the filter resisted the action of boiling water, and exhibited a very hard horny matter.

This disproved.

It remains to be examined, therefore, whether the sulphuric acid, or the starch itself, be decomposed.

Is the acid, or the starch, decomposed?

To judge by the letter from Petersburgh, the Russian chemists seem to suppose, that a decomposition of the sulphuric acid takes place.

The Russians seem to think the former.

To account for these phenomena, we should operate in close vessels. Accordingly, the author introduced into a tubulated receiver

Experiment

with sugar of milk.

receiver a hundred grammes of sugar of milk, four of sulphuric acid, and four hundred of water. To the neck of the retort was adapted a tubulated receiver, from which proceeded a sigmoid tube, opening under a jar filled with water.

After boiling for three hours, no gas had come over, except the air contained in the vessels. A piece of blue paper introduced into the neck of the retort was not reddened. The water that had passed into the receiver was without taste, did not redden litmus paper, had no smell of sulphurous acid, and did not precipitate lime-water, muriate of barytes, or acetate of lead; consequently it contained no sulphurous, sulphuric, acetic, or carbonic acid; in short, it was nothing but pure water.

Barytes-water traversed by the bubbles, extricated during the process, was not rendered turbid in the least, and the gas that had passed into the jars was nothing but the air of the vessels.

The sugar of milk decomposed, not the acid.

It is evident, that the sulphuric acid had not undergone the slightest decomposition: nevertheless, the sugar of milk was decomposed; it had a much more saccharine taste, and after saturation with chalk it fermented very readily with yeast.

Experiment repeated in close vessels.

It was necessary, therefore, to examine the decomposing action of the sulphuric acid on the substances in question. For this purpose the same experiment was begun afresh in close vessels, with 100 grs. of sugar of milk, 400 grs. of water, and 4 grs. of sulphuric acid. During the process no gas was evolved, as in the preceding experiment.

The liquid was then concentrated in a dish accurately weighed, after having added 5 grs. of potash to saturate the acid.

The mass thus evaporated to dryness should have weighed 109 grs. in consequence of the 100 grs. of sugar of milk, 4 grs. of sulphuric acid, and 5 grs. of potash employed; but it weighed only 98 grs. consequently there was a loss of 11 grs. This experiment was repeated twice more, and there was still a loss of 9 or 11 grs. giving a mean of 10 grs.

This loss is too great to be ascribed to any errour in the weighing, which was conducted with the greatest care.

appa-
formed.

Hence we must conclude, that this diminution of weight is occasioned by a quantity of water formed at the expense of the sugar of milk; and this with the more reason, as no gas, no acid, and no other volatile substance, was extricated during the boiling.

Starch afforded similar results.

All these experiments with the sugar of milk were equally repeated with starch except that a much larger quantity of water was added to prevent it from burning. The results were the same as those obtained with sugar of milk.

Conclusions.

From all that has been said, it follows :

General conclusions.

1. That starch and the fecula of potatoes, boiled with water acidulated with sulphuric acid, are converted into a liquid saccharine matter, the quantity of which corresponds with the weight of the starch employed.

2. That this saccharine matter is susceptible of the alcoholic fermentation.

3. That the sirup of starch is composed of gummy matter and saccharine matter in variable proportions.

4. That the sirup evaporated slowly in a stove exhibits an elastic substance, perfectly transparent.

5. That the gummy matter exhibits all the characters of a true gum, except that of forming mucous acid by means of the nitric.

6. That neither this gum, nor the saccharine matter, holds sulphuric acid in combination.

7. That the heat of boiling water alone is insufficient to convert starch into saccharine matter, as nothing is obtained but a bitter matter, and a horny substance insoluble in boiling water.

8. That sugar of milk treated with two, three, four, or five hundredths of sulphuric acid is converted into confused crystals, which have an extremely saccharine taste, and are susceptible of the alcoholic fermentation.

9. That this saccharine matter does not contain any sulphuric acid in combination.

10. That the muriatic acid effects the same changes in sugar of milk.

11. That neither the nitric nor acetic acid converts sugar of milk into fermentable sugar.

12. That sugar of milk thus converted into fermentable sugar becomes very soluble in alcohol.

13. That sulphuric acid is not decomposed in its action on starch and sugar of milk: and that, from the facts mentioned, it is much more probable, that the acid takes from these substances oxigen and hidrogen in the proportions necessary to form water.

VII.

Abstract of a Paper on the Deliquescence of Bodies; by Mr. GAY-LUSSAC*.

Deliquescence reducible to general principles.

ON the 17th of May [1812] I communicated to the Society of Arcueil some observations on the property that bodies have of attracting the moisture of the air, and which is more particularly designated in chemistry by the name of deliquescence. This property, hitherto badly analysed, may be reduced to general principles, by which we may easily ascertain what bodies possess it, the variations it undergoes according to the temperature, and the degree of the hygrometer at which it begins to manifest itself.

Owing to the attraction of a substance for water.

Mode of ascertaining it.

Exists where it had not been suspected.

As the deliquescence of a body is owing to its affinity for water, and as the effect of this affinity is to diminish to a certain degree the elastic force of the vapour contained in a determinate volume of air, it is very essential, both for knowing whether deliquescence can take place, and for obtaining comparable results, to place each body in an atmosphere completely saturated with moisture. Thus we find, that muriate of soda, sugar, &c., are very deliquescent; and that even nitre, and many other substances, in which this property had not been observed, possess it more or less.

Temperature to be attended to.

We cannot thus ascertain the degree in which a substance is deliquescent: but, to accomplish this, we must first observe, that, the deliquescence of a substance depending on its affinity for water, and this affinity itself being strikingly modified by heat, it is necessary to consider each temperature in particular.

Method of finding the de-

Suppose then a given substance, solid or liquid; and we wish to know its degree of deliquescence in an air saturated

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* Ann. de Chim. vol. LXXXII, p. 171.

with

gree in which a substance is deliquescent.

with moisture at 15° of the centigrade thermometer [59° F.]. If it be solid, first make a saturated solution of it in water at 15° [59° F.], and boil the solution*. If it boil at 100° [212° F.] the boiling point of pure water, the substance is not deliquescent: but if it do not boil at so low a degree, it is more deliquescent in proportion as the boiling point rises higher above 100°. Thus muriate of soda will be very deliquescent in air saturated with moisture, for its solution in water at 15° [59° F.] will not boil below 107·4°. [225·32° F.]. Nitre, too, will be deliquescent, but much less than the preceding salt, as its solution at 15° boils at 101·4° [214·52 F.].

The theory agrees with experiment.

Treatment of slightly deliquescent substances.

Experiment here perfectly agrees with the theory; but to have a good view of the deliquescence of nitre, and of all substances like it feebly deliquescent, they must be taken in small separate parcels: in this state they will be found to dissolve completely, while large crystals would only be covered with a liquid stratum, or would dissolve very slowly.

Importance of temperature.

It is easy now to perceive how important it is to attend to the temperature; for as heat greatly favours the combination of salts with water, the boiling point of each solution will vary according to the temperature at which it is made. Thus nitre, which is but slightly deliquescent at 15°, the saturated solution of which boils at 101·4, would be greatly so at the temperature of 100° [212°], as the solution saturated at this temperature would boil only at 110° or 112° [230° or 233·6° F.]

Nitre:

Salts not deliquescent.

Acetate of lead and corrosive sublimate do not perceptibly retard the boiling of water; and accordingly they are not at all deliquescent.

Liquids boil at a lower temperature in

In ascertaining the boiling point of saline or acid liquors, I observed a singular phenomenon, that deserves to be made known. It consists in this, that water, or any other liquid, does metal, than in glass, unless this contain some insoluble powder.

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* I shall here observe, that, instead of taking the boiling point of each liquid, it would be more accurate to take the force of its vapour at the temperature at which we would determine its degree of deliquescence, because the elevation of the boiling point is not proportional to the quantity of salt held in solution. Similar means should necessarily be employed to know the force with which solids attract the vapour of water, without any change in their state ensuing, as would take place with lime and salts deprived of their water of crystallization. This subject is treated at length in my original paper.

This may affect the graduation of thermometers.

does not boil so soon in a glass vessel as in one of metal, unless filings of iron, copper, or some other metal, powdered charcoal, or pounded glass, be put into the former. The difference of temperature for water reaches to 1·3° [2·34° F.], and sometimes even more. This fact is of the more importance in the graduation of thermometers, as we may observe a similar difference between two of these instruments made with equal care, but the upper points of which were taken one in a glass, the other in a metallic vessel. It is true, that if care be taken not to immerse the ball of the thermometer in water, the difference will be less.

No salt lowers the boiling point.

I have found too, that no salt possesses the property of lowering the boiling point of water, though Mr. Achard has asserted the contrary.

Degree of the hygrometer at which a salt will deliquesce ascertainably.

Knowing the boiling point of each solution, by means of which we have a measure of the deliquescence of the salt, and of its affinity for water, we may go farther, and ascertain the degree of the hygrometer at which deliquescence begins to take place. All that is necessary is to place the hygrometer under a jar moistened with the saline solution, and observe the degree it will point out at the expiration of a few hours. Thus it will be found, that with a solution of muriate of soda, saturated at 15° [59°], the hygrometer will stop at 90°; with a solution of nitre made at the same temperature, it will stop at 97°, or thereabout, &c.

Muriate of soda.

Sulphuric acid absorbs more than 15 times its weight of water.

Hence we may conclude, that muriate of soda will not be deliquescent below 90° of the hygrometer, but will begin to be so at this point, and become much more deliquescent beyond it. When a table indicating the degrees of the hygrometer corresponding to the boiling point of a certain number of salts is constructed, we may determine the degree of the hygrometer at which all the others will begin to be deliquescent, as soon as we know the boiling points of their aqueous solutions. I need not observe, that what is applicable to deliquescent salts is likewise so to all the solid or liquid bodies that have [illegible] affinity for water. On these principles we shall find, that concentrated sulphuric acid is capable of taking more than fifteen times its weight of water from air completely moist. In setting out from this property of various saline solutions having different degrees of elasticity at the same temperature, it is easy to determine with precision for every temperature, and every degree of the grometer, the quantity of vapour contained in a given f air; which Saussure could not do, notwithstanding y, on account of the imperfection of his processes.

Quantity of vapour in air ascertainable.

ethod, which I have already pointed out, consists in ids, from which nothing but water is separated by boiling them at very different temperatures; for instance, sulphuric acid more or less diluted; placing the hygrometer underneath jars wetted with each of these liquids; and the degree at which it stands. On the one hand we m my experiments the density of aqueous vapour, to that of air as ten to sixteen; on the other we know ing point, or elasticity of each liquid enclosed under a the hygrometer: consequently we have all the necessary for the solution of the problem in question. On this present employed, and I trust it will not prove uninto the science of hygrometry.

Process.

The author employed on it.

VIII.

the Correspondence between Dr. Bostock and Dr. cet, on the subject of the uncombined Alkali in the ani- Fluids. In a letter from George Pearson, M. D. R. &c.

To William Nicholson, Esq.

George Street, Hanover Square, Oct. 27, 1812.

SIR,

your Journal for the present month I read the letter of Dr. Marcet, addressed to his friend, Dr. Bostock; in which rs the evidence of some experiments to prove, that the which exists in the animal fluids is in the state of muriat ate), and "that the whole of the uncombined alkali is ."

Introduction, by reference to Dr. Marcet's letter to Dr. Bostock, and the remarks thereon by Dr. B.

appears, that Dr. Bostock was of opinion, that the sup uncombined alkali was potash, according to my proonal conclusions, and not soda: but on the representation evidence just mentioned, he has changed his opinion, and s become the vehicle of Dr. Marcet's letter to the public;

portion, probably not one fourth of a grain, or at most half a grain of alkali, in the whole mass; and this by fusion might be united to form a compound unknown.

Objections to Dr. Marcet's inference.

To the inferences of my adversary I also object. 1. That it is assumed, without testimony, that alcohol dissolves a large proportion of muriate of potash. It is, I believe, admitted, that this menstruum dissolves none at all; but if this be an errour, I demand the proof.

2. It was not admitted, as I reasoned, that acetate of soda is nondeliquescent; and therefore the proof I offered of the alkali being potash from the deliquescent property of the acetate was eagerly seized to expose my ignorance, by exultingly exclaiming, that I had committed *a palpable errour*. I acknowledge, that I had taken for granted, with most chemists, what I subsequently admitted was not a fact; but I am now in a doubtful state of mind, with regard to this property; for professor Berzelius confidently assures me, that acetate of soda was found, by repeated experiments, to be uniformly nondeliquescent: and on observing, that in my experiment I had found it otherwise, we agreed, that probably the different results were owing to the soda I used containing a proportion, however minute, of potash, and which I could not perceive by tartaric acid, whereas that he employed was exempt. If this be true, it will be a stronger proof, that the alkali is potash, than the united testimonies produced to prove that it is soda.

3. Dr. Marcet argues, that from principle it may be inferred, that soda, and not potash, is the impregnating alkali, because the latter attracts muriatic acid more strongly than the former. This is true in the circumstance of single elective attraction; but any reasoning from this law, when more than one menstruum is present, and two or more bases, is fallacious; especially when the different substances present are not certainly known. And here I must observe, that I have never contemplated potash as existing in an uncombined state in the animal fluids, but in reality in combination with a destructible acid, or with animal oxide. This acid, from some trials I was inclined to propose, is the malic acid; but I did not venture to offer it to notice, although I did not abandon the notion: however, I find from the conversations of professor Berzelius, now in London, that he coincided with me in an analogous, if not a simi-

lar result. "You very nearly," said he, "made a capital discovery; for I have ascertained, that it is *the lactic acid* in union with the alkali of the animal fluids." I hope the British public will soon be edified by the translation from the Swedish language of a work of this most acute chemist, and, as I hear, by a most able editor. Hence much light will be afforded, especially in animal chemistry. This fact is, however, only within the record before us, to repel any *a priori* conclusion from a case of simple elective attraction. I had long considered the case of this kind noticed by my opponent, for it was too glaring to be passed by. If reasoning from principle could be depended upon, I would argue; that, as all animals either immediately or mediately live upon vegetables, and as vegetables very generally contain potash combined with acids, or other things destructible by fire, it is reasonable to conclude, that the fluids of animals must be impregnated with potash in such a state of combination.

I know it has been argued by some able chemists, that the potash must be united to muriatic or sulphuric acid, and soda must be united to some weaker acid, such as carbonic, lactic, acetous, malic, &c., agreeably to the assumed law, that the stronger menstruum unites with the stronger basis, and the weaker menstruum with the weaker basis. But there are so many exceptions to this rule, that it cannot be justly termed a law.

Mere general statement of the result inadmissible testimony.

Lastly, in his P. S. Dr. Marcet says he has instituted the process above examined on a very large scale, on a large quantity, some gallons, of bullocks' blood, with the same results as on small quantities of animal matter. I believe such evidence is inadmissible; for if mere general statements of results be received as testimony, much errour will be liable to be introduced; as the public in these cases cannot be in possession of the means of repeating the experiments, and judging of their accuracy. It is to be regretted that the author did not render his experiments instructive, by the necessary detail; however, if they were a mere repetition of former ones, the questionable fact would still remain undetermined.

The chemical world may now perhaps be furnished with the means of judging whether or not Dr. Marcet has removed every shadow of doubt by legitimate inductive reasoning.

Authority no proof.

My opponent, not content with proofs by experiment, has endeavoured to command assent by a most respectable authority of opinion. But *Truth* is not the daughter of mere human authority, but of *Time*, producing testimonies of sense and of reason.

I beg to have permission to make a very few remarks, which, although justifiable, yet being personal, will afford but lenten entertainment, and still less instruction, to the public. In making this authority the vehicle of his letter, my opponent thinks proper to express disapprobation of my mode of controversy, and to more than insinuate, I should not have been honoured with further notice, but for the "interference" of his friend. Accordingly, but for this fortunate circumstance the public would not have been instructed by his letter before us. This conduct, I own, I think is rather selfish; for a public-spirited man will always make sacrifices of his feelings for the benefit of the republic. It is, however, good, that the *interference* overcame the resolution after four months' obstinate resistance. The head and front of my offending was, it seems, to the extent of an attempt to be jocular, in which I never meant to inflict any wound on the feelings. It grieves me, certainly, to find, that some of my expressions were construed, insidiousness—"Non vulnera fidelia amantis, sed oscula blandosa malignantis." In the endeavour to expose the inefficiency of the proposed method of investigation, and to honour illustrious chemists, whose successful methods were unworthily disvalued, I preferred the manner of controversy complained of, to the alternative—a serious remonstrance. For as my affectionate friend, the Prince of Philologists, (now no more!) was wont to say, "Cantantes minus via lædit."

Reasons why the author preferred a jocular tone.

In conclusion; I would fain hope, that, if this warfare must be continued, special care will be employed, that nothing be said or arise, which can reasonably excite* painful sensations in either party. And if it be agreed, that our axioms and conclusions are but inductive reasonings, according* to the known facts, which therefore are liable to be subverted by the facts being multiplied, whatever be the issue, no humiliation ought to be experienced, as the parties will moult no feather.

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* I have no where charged Dr. M. in the terms alleged, that he had committed *blunders*. I can well spare the word blunder from my vocabulary, having little use for it, although by the law of retaliation amply justifiable.

I have the honour to be,
Dear Sir,
Your most faithful Servant,
GEORGE PEARSON.

IX.

On Hygrology, Hygrometry, and their Connexions with the Phenomena observed in the Atmosphere. By J. A. De Luc, *Esq.* F. R. S.

To W. Nicholson, Esq.

SIR,

Atmospheric phenomena important to the knowledge of the nature of water:

IN the third part of my paper on the *electric column*, published in your Number 124, for December, 1810, where I have considered that instrument as an *aerial electroscope*, I have shown the importance of studying all the *atmospheric* phenomena, before a final decision could be obtained of the question agitated for some time, on the *nature* of *water*; whether it is a *compound* or a *simple* substance; a question which embraces the whole *theory of chemistry*. Especially I hope to have made it evident in that paper, that, since *atmospheric* phenomena are to be considered in the solution of the above question, we ought to study particularly all those of the *electric fluid* in the atmosphere; to which we might be led by the phenomena of the *aerial electroscope*, provided we did not connect them with arbitrary hypotheses, nor forget to take into consideration the *nature* of the *electric fluid*, which, from the great phenomena of *lightning* and *thunder*, has evidently a great share in *meteorological* appearances. My papers, Sir,

and particularly those connected with electricity.

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* Experientiæ ordo primò lumen accendit, deinde per lumen iter demonstrat, incipiendo ab experientia ordinata et digesta, atque ex ea educendo axiomata, atque ex axiomatibus constitutis rursus nova experimenta.—Nov. Organum F. Baconis.

in the last numbers of your Journal, were destined to show, from our own experiments and atmospherical observations, what are the *nature* of the *electric fluid*, and its interference in meteorological phenomena; and I now come again to the same subject, under another point of view.

Rain not from moisture in the air:

but the ponderable part of the air itself.

1. My observations of the *aerial electroscope*, published in your No. 124, show, that the changes in the phenomena exhibited by this instrument have no connexion with the state of *moisture* in the *ambient air*. I proved also, in the same paper, this important point in meteorology, that *rain* does not proceed from a *moisture* actually existing in the atmosphere. This, if it be certain, overturns the new *theory of chemistry*; for thus *rain* cannot proceed from any other cause than that of a *decomposition* of the *atmospheric air* itself, a fluid *sui generis*, the *ponderable* part of which must be *water*.

Grounds of the conclusion.

Can the hygrometer be depended on?

2. But this conclusion rested on the indications of the *hygrometer*, Mr. De Saussure's observations, and my own, on high mountains; in the very region of the atmosphere we saw the *clouds* forming around us, and pouring *rain*, while an instant before our *hygrometers* testified, that there was very little *moisture* in the *air*. But here a question arises: is the *hygrometer* an instrument to be depended upon, for the purpose of indicating the real quantity of *moisture*, or *evaporated water*, mixed with the *air*, in the place where it is observed?

This an important question.

3. This, Sir, is a very important question, as well in natural, as in experimental philosophy; and I wish, through your valuable Journal, to attract the attention of your readers to this instrument. I had very little hope of success on this point, when I wrote my preceding papers in your Journal; because, from a circumstance which I shall explain hereafter, none of my *hygrometers* could be found; but it is not the case now.

Progress made in the inquiry.

4. I had already made some progress in the correspondent researches of the indications of the *hygrometer*, and the phenomena of *rain* and *fair weather*, when, in 1786, I published in London my work, *Idées sur la Météorologie**; but I had carried them much farther, when I delivered to the Royal Society my papers on *hygrology* and *hygrometry*, published

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* This work may be had of Messrs. Dulau and Co. booksellers in Soho Square.

in the *Phil. Transactions* for 1790 and 1791; the subjects of which I shall here shortly explain, for those of your readers who do not possess the *Phil. Trans.*

The hygrometer a perfect measurer of moisture in the air.

5. There is no physical instrument, the name of which terminates in *meter*, as used for *measuring* the *intensity* of the *cause* acting upon it, so deserving that name, as the *hygrometer* described in these papers; for this instrument alone has the property of measuring the *whole extent* of the cause which influences it; which *extent* is comprised between two natural and opposite *extreme points*, one of which I shall first describe: it is *extreme dryness*, or absence of all *moisture*; which, therefore, is an absolute 0. I have proved, in the above papers to the Royal Society, that this point is effectually obtained, by placing the *hygrometer* in a close vessel, filled previously with a sufficient quantity of fresh calcined *lime*, taken *red-hot* from the kiln.

Point of extreme dryness.

Principle on which the ascertaining it is founded.

6. The principle which led me to this method is, that, *evaporation* being produced by *heat*, if *red-heat* is not destructive of a *hygroscopic body*, it must occasion the *evaporation* of all the *uncombined water* the latter contains in its pores. And by previous experiments on various bodies of that kind, I found, that *lime*, passing from *red-heat* to *extreme moisture*, increased in proportion of nearly *half* its *weight*. I fixed therefore, upon *lime*, and I employed a large vessel, which I filled with *red-hot lime*. When it was cool, that vessel having at the top small openings for introducing the *hygrometers*, (after which they were closed, and opened only for taking them out,) I took thus the point 0 on a great number of various sorts of *hygrometers*, of which I shall speak hereafter. I have described this vessel in the Phil. Transactions; it is cylindrical, 1 foot diameter, and 3 feet high; I have it still, and when I place in it one of the *hygrometers*, the 0 of which had been fixed in it 10 years ago, I do not find any sensible difference in this point. Thus, therefore, the point of *extreme dryness* is perfectly ascertained.

Not varied by time.

Point of extreme moisture.

7. As to the opposite point, that of *extreme moisture*, I have proved in the same paper, that it was surely obtained by immersing the *hygrometer* into *water*; where it soon attains a point, beyond which it does not go, whatever length of time it remains

Scale.

remains there. This point I have called 100, and the scale is divided into 100 parts.

Inquiry after the fittest substance for hygrometers.

8. Another important object treated in the same paper, and which occasioned me much labour, was, of what substance the *hygrometer* should be constructed. On this particular point I related a long series of experiments, occasioned by the first results I obtained by trying many kinds of *animal* and *vegetable* substances: some of which could be used in thin *threads*, torn in the *length* of their *fibres*; and also in thin *slips* cut *across* the *fibres*. Now, I found, that when used in the *length of the fibres*, their *lengthening* by *moisture decreased*, and at last they were even *shortened*, while the same substances cut across the fibres continued to *lengthen*: which at first embarrassed me very much*.

Exp. to find whether substances cut lengthwise imbibe moisture while shortening.

9. I could not decide immediately from these observations, whether the substances taken in *length* continued to imbibe moisture, while, however, their *length* was *decreasing*; and in order to ascertain this necessary point, I contrived a vessel, described in the same paper. In that vessel I enclosed together several pieces of *hygrometers*, made of the same substances; in one, cut in the length, and in the other *across* the *fibres*; and a balance, indicating the 500th part of a grain, to which I

[illegible]

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* The cause of the difference in the successive expansion by moisture of the same fibrous substances, taken in the *length* and *across* their *fibres*, proceeds from the nature of these substances. The main *fibres* in their length are united by a web, which are seen when we split these bodies. [illegible] over the larger ones a sort of *meshes*, similar to those of a net. The first effect of *moisture* is on the *longitudinal* fibres, which it lengthens; but when it penetrates the *meshes*, it *widens* them, and thus shortens the fibres; as the *length* of a *net* is *lessened* by stretching it across. Moisture therefore acts in two opposite ways on the *fibrous* substances, and in a degree differently in its progress on the same substance, and differently also in different substances. And besides, the whole lengthening is very small in all of them. Now, one of these effects is suppressed by taking the same substances *across* the *fibres*, namely, that which acts on the *length* of the latter; there remains only that which acts on the *width* of the *meshes*, which, if not absolutely proportional to the increase of *moisture*, is never in an opposite sense. Besides, there is a great gain with respect to the extent of the lengthening, and therefore in the degrees of the *hygrometer*; for instance, a *slip of whalebone*, by passing from *extreme dryness* to *extreme moisture*, increases nearly one eighth in length.

suspended very thin *shavings* of the same substances as the enclosed *hygrometers*; which *shavings* indicated, by the increase of their *weight*, the *weight* of the *water* which penetrated them. I had a *lime-vessel* by which I first produced *extreme dryness* in the vessel containing the instruments; and when I had observed them in that state, and taken off the vessel containing the lime, I had also a manner of increasing *moisture* by degrees in that of the instruments, observing at each step the motions of the *hygrometers*, and the increase of weight of the *shavings*.

Results.

10. The general results of this experiment were the following:—1. That substances taken in *length* continue to imbibe *moisture*, though they cease to *lengthen*, and some even begin to *shorten*. 2. That *slips* cut *across* the *fibres* continue to *lengthen* so long as the *moisture* increases. 3. That the *slip* of *whalebone* follows very nearly in its *lengthening* the rate of the increase of *moisture*, indicated by the increase of *weight* in its *shavings*. From this last result, and from the great elasticity of this substance, which makes it always sensibly return to the same *length* with the same *degree* of *moisture*, I fixed on a *slip of whalebone* for my *hygrometer*.

Whalebone preferable.

The instrument little known:

11. Such was the point which I had attained, when I delivered my papers to the Royal Society; thus concluded by the determination of an absolute and comparable *hygrometer*, which was wanting in the set of meteorological instruments commonly observed: but by an unlucky circumstance, it still remains little known, and thus enters very seldom into the considerations concerning meteorological systems. I had directed, in the construction of that instrument, a very able German instrument-maker in London, Mr. Haas; but after he had sold a few, he was engaged to go to Portugal, with a pension from the government; and since that time, no other instrument-maker had undertaken to construct it. But lately a Hanoverian gentleman, Mr. Hausmann, who lives now at Cumberland lodge, near Windsor, seeing that it was a very important instrument for meteorology, has undertaken its construction, and having succeeded, he is disposed to make it for those experimental philosophers, who may wish to have it.

but now may be bought.

The quantity of water in air answering to

12. So far, however, as may be seen in the above account of these experiments, I had only obtained a *ratio* between the

quantities

given degrees of the hygrometer still remained to be found.

quantities of *moisture*, and the *degrees* of my *hygrometer*; or what *part* each *degree* was of the *whole*: but I had not obtained a knowledge of the *absolute* quantity of *evaporated water*, which, in a given bulk of *air*, corresponded to these *degrees*; a knowledge very essential in the investigation of the *cause* of rain. I saw that this was at least necessary for obtaining more certainty in meteorological conclusions. I relied in this respect on Mr. De Saussure's experiments, as I had not yet had time to undertake them myself; but I thought then to repeat the same experiments, for the following reasons.

Mr. De Saussure's experiments objected to.

13. Mr. De Saussure had made these experiments with his *hair-hygrometer*, which was so dissimilar to mine in the rate of *lengthening* with the same *increases* of *moisture*, that his results could not be immediately applied to my instrument. But especially, he had made all these observations in the course of one day; so that he could only obtain a few immediate points of comparison, whence he deduced a general law of the correspondence of the *degrees* of his *hygrometer* with the *quantities* of *evaporated water* in a given *bulk of air*. This was a first reason why some natural philosophers did not admit the results of his experiments. There were also some other reasons, which I shall hereafter mention: but these results were so important in meteorology, as he himself explained, that I resolved to repeat the same experiments in such a manner, as to remove all the objections, which I clearly saw could only affect the exactness of his experiments, but not their main results. I shall now mention all these objections, and the manner in which I proposed to remove them.

The author resolved to repeat them.

1st. objection.

14. The first objection, as I have said above, was the short time employed in his experiments, to which he had been obliged by the nature of his vessel: I therefore wanted to use a vessel in which I could prolong these operations as long as I should find it necessary. A second objection had been made against the manner in which he first produced *extreme dryness* in his vessel, which was by new-calcined *salt of tartar*; a substance which has *chemical affinities* with *water*, and might absorb *air* with it: I wanted therefore to use new-calcined *lime*, as I had used it for fixing the point of *extreme dryness* on my *hygrometers*. Lastly, there was an objection against the manner by which he had determined the *quantities* of *evaporated water*

2nd. objection.

3d. objection.

water in his vessel: it certainly could not be very exact; but it was sufficiently so, for the final and most important conclusions of a first attempt of these experiments. However, these objections had rendered the greatest number of experimental philosophers inattentive to this great step concerning meteorology, so that it was almost forgotten. This was my first motive for undertaking the same experiments with the precautions above explained.

The experiments difficult.

15. I found this attempt much more difficult than I had expected; for it cost me more than two years in useless trials, for obtaining, first, a vessel which would remain *air-tight* during all the time that these experiments should require. At last, however, I succeeded, and the experiments themselves took me afterward more than one year. These experiments are related in a work which I published at Paris, in 1803, under the title of *Traité élémentaire sur les Fluides expansibles:* but on account of the present circumstances of Europe, and this work being in French, a few copies only are come to England. This, Sir, makes me desirous to consign to your Journal a short account of these experiments.

Related in a French work.

Object of the experiments.

16. My purpose was to ascertain what quantities of *evaporated water* in a known *space of air* corresponded to each *degree* of my *hygrometer*; and I determined, that this *space* should be one *cubic foot*. My first success in overcoming the difficulties was that of obtaining a vessel, which would remain *air-tight* during the whole course of these experiments. I found, that no vessel could be rendered air-tight so long, which had a large opening at the top; and that therefore this opening should be only what was necessary to introduce the instruments into it. I then procured a *glass vessel*, about 23 inches high, and 8½ in diameter, the opening of which at the top was only 2½ inches in diameter. I measured the *capacity* of this vessel; it was not quite one *cubic foot*; but I ascertained the differences to which I was to proportionate the quantities of *evaporated water*, so that they might be as 1 *grain* in a *cubic foot*.

Vessel for making them in.

Method of ascertaining the quantity of water evaporated in a vessel with certainty.

17. Before that time, I had found a sure method of ascertaining the quantities of *water* successively evaporated in a vessel, without opening it; in order to prevent any exchange of the internal with the external air, lest the latter should intro-

duce some moisture with it. This method was to enclose equal quantities of *water* in very thin and small *glass bubbles*, with a neck drawn to a very small point, easily sealed with the flame of a taper; and before this last operation, I determined the quantity of *water* that each contained, by a beam which indicated 1000th part of a *grain*. These *glass bubbles* were placed in the upper part of the vessel on a circular stand, and I had, outwards at the top, a mechanism for breaking them without opening the vessel. This method I applied to the glass vessel above mentioned.

Mr. de Saussure showed, that rain is not condensed by cold.

18. Such were the means which I employed for ascertaining the quantities of *evaporated water* in a *cubic foot* of air, acting on the enclosed *hygrometer*. But these experiments required another condition, which Mr. de Saussure had already introduced in them: because those natural philosophers, who attributed *rain* to the *moisture* in the atmosphere, had supposed, that this *moisture* was condensated by *cold*. Mr. De Saussure had sufficiently proved, that it was not the case, by observing the effects of the changes of *temperature* on his enclosed *hygrometer*. I was therefore to introduce the same condition in my experiment, and for this purpose I enclosed also in my vessel a *thermometer* with Fahrenheit's scale. Lastly, as I intended to make the same observations on every successive *grain* of *evaporated water*, which would take a very long time; having previously found that *extreme moisture* was produced in the vessel by a small number of *grains* of *water*; and even that they could not undergo great changes in the degree of *heat*, without some *water* being deposited in the sides of the vessel: this obliged me, in order to obtain the same temperatures in the observations of the effects of each successive *grain* of evaporated water in the vessel, to make these experiments only in the spring and the autumn; because, in these seasons, I could obtain naturally almost every day in my room the temperatures of 50, 55, 60 of Fahrenheit, on which I fixed for all these experiments. By this method I was sure, that the temperature would be always the same in every part of the vessel it being that of the air in the room.

Thermometer enclosed in the vessel.

Extreme moisture produced by a few grains of water to a cubic foot of air.

The experiments confined to spring and autumn for uniformity of temperature.

Two series of experiments made.

19. I made two series of these experiments; one beginning in the autumn of 1795, and ending in January, 1796; the other beginning in the autumn of 1796, and terminating in

February, 1797: each of them began by producing *extreme dryness* in the vessel, and proceeded by the *evaporation* of successive grains of water; observing afterward the changes produced on the *hygrometer* at the three fixed *temperatures*. In the course of these experiments I had a proof, that the vessel remained *air-tight*. For in order to ascertain the effects of the increase of water at the three temperatures, I consecrated many days, even weeks, to the observation of each step, by repeating it many times; which made both sets of experiments last near 6 months: however, I found no sensible difference in these observations from the first to the last day, with every quantity of water; and in ending them, I had an immediate proof, which it would be too long here to explain, that the *aqueous vapour*, which had been produced in the vessels, had added its expansibility to that of the *air* originally enclosed in it.

The vessel remained air-tight.

The experiments many times repeated.

Rules of hygrometry deduced from the experiments.

20. This, I think, was a complete determination of the correspondence between the *degrees* of my *hygrometer*, and the *quantities* of *evaporated water* in one *cubic foot* of air, at the observed degrees of *heat*. I then undertook to derive from these experiments general rules of *hygrometry*. These deductions begin at p. 325, of the 2d vol. of the above-mentioned work; they are given in 13 successive tables, of which I shall only mention two.

21. In *table* ii. are united the results of both experiments, (which differ very little from each other), reduced to their mean terms. Each set began at the point of *extreme dryness* in the vessel; a point where the *hygrometer* stood at 0 in both. At that point, no *moisture* being in the vessel, the change of *heat* from 50 to 60 of Fahr. produced no change in the *hygrometer*. During both sets of experiments, the limits of the *evaporation* in the vessel were the same: 5 *grains* only of *water* could remain *evaporated* at the temperature of 50; 6 *grains* at that of 55, and 7 *grains* at 60. Beyond these quantities, at the respective *temperatures*, a certain quantity of *water* was deposited on the sides of the vessel in the form of *dew*; but when this effect took place at the temperature of 50, the *dew* was dissipated when the *heat* of my room came to 55; and when it happened at 55, it was dissipated when the *heat* in the room arrived at 60.

Limits of evaporation at different temperatures.

22. Thus therefore we have the natural *limits* of the quantities

ties of *evaporated water* that can subsist in one *cubic foot* of *air* with these three degrees of heat; but by the rate of its progress, this correspondence may be continued to higher and lower temperatures, as I shall explain, after the following indication of the immediate effects observed on the *hygrometer* of each increase of 1 *grain* at the three *temperatures*. In the first two columns of the table, the points of the *hygrometer* cease to be indicated at the period when *dew* appeared on the side of the vessel.

Table of moisture indicated by the hygrometer at different temperatures.

Grs. of water in 1 cubic foot.	Points of the hygr. at temp. 50°.	Points of the hygr. at temp. 55°.	Points of the hygr. at temp. 60°.
	°	°	°
1	15·2	14·5	13·9
2	29·9	28·5	27·6
3	51·6	47·2	43·2
4	74·9	64·1	55·
5	89·8	78·6	68·3
6		93·9	82·1
7			96·6

Remarks on this table.

23. This table shows the progress of the effects on the *hygrometer* of the *evaporation* of the successive *grains* of water. These increases were stopped, as I have said above, by some water being deposited on the sides of the vessel. This effect took place for the 6th *grain* with the temperature 50°, and for the 7th *grain* at 55°: however, this happened only when the *grains* were entirely evaporated, during which time the *hygrometer* had moved; but there was no fixed point to be obtained correspondent to the new *grain* of *water*, since a part of it at last was deposited on the sides of the vessel.

Account of the other tables in the work.

24. The tables which follow this, in my work, serve to combine these results, by the rules of interpolation, for obtaining the intermediate terms not given by the experiments; and also to continue the same series, on one side, up to 98 of the *thermometer*, and on the other, for a particular purpose, down to 0. The table ix., which is the result of all these combinations, is construeted in such a manner, as to afford immediately the answer to the following questions, very important in meteorology.

Questions an-

1. A point having been observed on the *hygrometer* in the

open air, what are the quantities of *evaporated water* in one *cubic foot* of that *air*, at any given *temperature*?

swered by table ix.

II. The points of the *hygrometer* and *thermometer* having been observed, what is the quantity of *evaporated water* in one *cubic foot* of that part of the atmosphere?

III. The points of both instruments having been observed, to what degree ought the *thermometer* to fall, in order that the *hygrometer* should arrive in that air at 100; which point it must attain before there is any precipitation of *water*?

25. The answers to these questions, from the immediate results of my experiments, led to this first conclusion; that no diminution of *heat* in the atmosphere could occasion in it the precipitation of such a quantity of *water* as to produce *clouds* pouring *rain*; which confirmed me in the opinion already expressed in my work, *Idées sur la Météorologie*, that the *aqueous vapour*, constantly ascending in the atmosphere, ceased in great part to act on the *hygrometer*, being converted into an *aeriform fluid*, namely, the *atmospheric air*, and that *clouds* and *rain* were produced by the decomposition of this *fluid*.

No degree of cold in the air can produce clouds and rain.

Aqueous vapour ceases to act on the hygrometer from its being converted into atmospheric air.

26. Such was the conclusion of all the above *hygroscopic* experiments; and with respect to *atmospheric* phenomena, it coincided with the observations of Mr. de Saussure and myself in the high regions of the atmosphere. Having both long inhabited our mountainous country near the Alps, we had separately followed the same meteorological observations with our *hygrometers*, and we had absolutely ascertained these two points.—1. That the more we ascend in the atmosphere, the *dryer* the air is observed; and that even, in clear weather, it is *dryer* in the night than in the day. 3. That *clouds, rain, hail,* and *thunder*, are produced in certain *strata* of the atmosphere which were *clear* a moment before, and in which one *cubic foot* of *air* did not contain above *two grains* of *water*. Having both separately, at different times, and also in different parts of the mountains, made the same observations, and published them separately, I cannot suppose, that their results can be contested. Thus it is certain, that *rain* is *not* produced by a *moisture* existing in the atmosphere; and consequently that it proceeds from a *decomposition* of the *air* itself.

Conclusions from hygroscopic experiments agreed with the atmospheric phenomena of the Alps.

The same deductions formed separately by the author and Mr. de Saussure.

27. From what I have said so far, it may be judged, that the

Proofs, that

whole

the modern theory of chemistry is erroneous.

whole of this work was intended to prove, how erroneous was the modern *theory of chemistry*, the foundation of which is to suppose, that *water* is a compound of *two* substances, called by its authors *hidrogen* and *oxigen*, and that the *atmosphere* is principally composed of two *fluids* called by them *hidrogen air* and *oxigen air*; a system in which, for the explanation of the greatest atmospheric phenomena, which ought to have been their first objects of comparison, those of *clouds* and *rain*, they had been reduced to suppose a *condensation* of the *aqueous vapour* by *cold*, which supposition the above experiments prove to be absolutely erroneous. This is the only point, which I have here considered; and indeed it is sufficient to overturn the whole theory: but in other parts of the work I entered into the examination of all its parts, beginning with the original experiments from which the *composition* of *water* had been concluded; and in analysing these experiments I made it manifest, that, far from being satisfactory, there were many unwarrantable hypotheses to be made, in order to connect the *facts* with the *conclusion*.

Berthollet's attempt to defend it.

28. When my work had been published at Paris, Mr. BERTHOLLET, one of the authors of that *chemical theory*, attempted, in the *Annales de Chimie*, and in another French Journal, to defend the only resource of that theory, namely, that *rain* was the effect of the *condensation* by *cold* of the *aqueous vapour* existing in the atmosphere. He acknowledged however two points, first, that my experiments with respect to the effects of *evaporated water* on the *hygrometer* at different temperatures had been made with an uncommon accuracy; and that I had thus demonstrated the errour of those, who attributed *evaporation* to a *dissolution* of *water* by *air*. These were two important concessions; but being loth to abandon his theory, and totally unacquainted with meteorological phænomena, he attempted again, as it was absolutely necessary for the support of his theory, to explain *rain* by the *cold* condensating the *aqueous vapour* in the atmosphere; thinking that by transporting the condensation to *very high regions* of the air, no objection could be made from immediate facts: but he was mistaken; since Mr. de Saussure and myself had proved, from immediate facts, that the upper regions of the atmosphere are *dryer* than those that we can attain.

29. I

Answered by the author.

29. I answered, in the *Annales de Chimie*, to every part of Mr. Berthollet's objections; and neither himself, nor any other experimental philosopher, has ever replied; while, on the contrary, many have abandoned the fundamental part of that theory, the *composition* of *water*: and indeed, one of its first inventors, with whom, having seen his experiments, I had acquiesced in his conclusion, and for a time maintained it, I mean Dr. Priestley, made me himself abandon it, on account of new chemical results obtained in his experiments, which he opposed to Mr. Berthollet.

The composition of water given up by one of its first supporters.

We are too hasty in forming hypotheses.

30. I have been induced, Sir, to give you this abstract of a work little known in England, in order the more to fix the attention of natural philosophers on the *hygrometer*, of which I have thus proved the importance in natural science. It is difficult to abstain from making *theories* on the first phenomena we observe of a new kind, or from admitting those which appear probable to us; and I have said above, that I had at first acquiesced in that of the *composition of water*: but by the progress of experiments, new facts are discovered, and correct the *theories* too soon admitted. My long study of every branch of meteorology, being united with the experiments related in this paper, which indeed were directed to that object, have demonstrated to me this great point in natural philosophy—that it is impossible to attribute *rain* to a *moisture* actually existing in the atmosphere; which alone entirely refutes the new *chemical theory*. Moreover, all the experiments on the combinations of *gasses* with other bodies concur to show, that the *ponderable* part of these fluids is *water*. Lastly, in the above mentioned work I proved, as I have done succinctly, Sir, in my paper published in your Journal for December, 1810, that, when we consider the atmospheric air as an *aeriform fluid*, though never mixed but with a very small quantity of *aqueous vapour*, all the *atmospheric* phenomena are explained.

I may conclude, therefore, that *meteorology* makes an essential part of *natural philosophy*, and that it is not so obscure as it is commonly thought.

I have the honour to be,
Sir,
Your obedient, humble Servant,
J. A. DE LUC.

Windsor.

X.

METEOROLOGICAL JOURNAL.

1812.	Wind.	Barometer.			Thermometer.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
9th Mo.										
Sep. 27	S W	30·04	29·77	29·905	68	59	63·5	—	·04	☽
28	S E	29·90	29·67	29·785	63	53	58·0	—		
29	N E	29·90	29·90	29·900	57	55	56·0	—		
30	E	29·90	29·75	29·825	59	55	57·0	·20		
10th Mo.										
Oct. 1	W	29·92	29·75	29·835	62	48	55·0	—	·28	
2	W	29·98	29·92	29·950	64	48	56·0	—	·01	
3	S E	29·98	29·90	29·940	61	41	51·0	·02	·09	
4	S W	29·90	29·78	29·840	67	44	50·5	—		
5	N W	29·78	29·31	29.545	69	55	62·0	—	·23	●
6	S E	29·32	29·12	29·220	64	41	52·5	—	·22	
7	Var.	29·32	29·12	29·220	57	45	51·0	—	·05	
8	S E	29·40	29·32	29·360	62	40	51·0	—	·06	
9	N W	29·40	29·35	29·375	53	46	49·5	·13	·07	
10	S	29·35	29·28	29·315	55	43	49·5	—	·32	
11	W	29·28	29·23	29·2[illegible]5	53	38	45·5	—	·06	
12	Var.	29·23	29·16	29·195	51	37	47·0	—	·08	
13	S E	29·16	28·83	28·995	57	45	51·0	—	·60	☾
14	S W	29·14	28·83	29·985	51	42	46·5	·03	·10	
15	W	29·39	29·14	29·265	53	43	48·0	—	—	
16	W	29·56	29·39	29·475	56	34	45·0	—	—	
17	S W	29·56	29·05	29·305	55	39	47·0	—	·29	
18	W	29·17	28·74	28·995	61	50	55·5	—	·44	
19	S W	28·81	28·53	28·670	59	49	54·0	·09	·37	
20	N W	29·54	28·81	29·175	67	42	54·5	—	—	○
21	N W	29·74	29·50	29·620	53	37	45·0	—		
22	S W	29·45	29·35	29·400	59	46	52·5	—	·02	
23	W	29·92	29·45	29·685	54	41	47·5	·10		
24	S W	29·92	99·74	29·830	56	41	48·5	—	—	
25	S E	29·50	29·40	29·450	56	41	48·5	—	·31	
26	S W	29·74	29·72	29·730	53	38	45·5	·06	—	
		30·04	28·74	29·468	69	34	51·46	.63	3·64	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Ninth Month. 26. Windy. Some rain in the night. 28 a. m. Very foul sky. 28, 29, 30. Rain at intervals in very small quantity.

Tenth Month. 1. A thunder-storm about 1, p. m. which was chiefly in the W. with heavy showers. 4, 5. Much dew. A storm of wind about midnight on the 5th. 6. Windy. 7. Misty morning: the trees dripping. 8. Rainbow, several times repeated between 8 and 9, a. m. Showers followed. 10. Rainbow, p. m. 11, 12. Rain in the night, misty morning. 13. *Cirrostratus* and *Nimbus* a. m, sunshine, and showers: a wet night. 16. Sunshine, with *Cumulostratus.* 17. Misty morning. 18. Squally during the night, with heavy showers. 19. Thunder and lightning about 2 p. m. Very heavy squalls with rain. 20. Sunshine a. m. much wind. 21. Clear and calm this evening. 22. a. m. Overcast, windy. In the evening a wet squall, with some lightning. 24. No swallows have been since the 19th or 20th. 25. A few swallows appeared again to-day.

RESULTS.

Prevailing winds westerly.

Barometer: highest observation, 30·04 inches; lowest 28·74 inches;

Mean of the period 29·468 inches.

Thermometer: highest observation 69°; lowest 34°.

Mean of the period 51·46°.

Evaporation 0·63 inches. Rain 3·64 inches.

The evaporation was much greater, during the above period than the amount here stated; as appears by observations as the Laboratory. It was probably not less than 2 inches. The situation of the guage had been changed.

Plaistow, L. HOWARD.
Eleventh Month, 18, 1812.

XI.

On the Nature and Detection of the different Metallic Poisons. In a Letter from Mr. CHARLES SYLVESTER. *To William Nicholson, Esq.*

DEAR SIR,

Nature and detection of the different metallic poisons, a subject of inquiry.

ALTHOUGH an inquiry relative to the nature and detection of the different metallic poisons would need no apology, at any period, for its introduction to the readers of your Journal, yet I feel it to be the less necessary, at the present moment, from the great interest which has, of late, been manifested for investigations of this nature; and am induced, therefore, to send you the following essay, which was, for the most part, prepared for a particular purpose many months ago. Should it tend to lessen any of the difficulties attendant on a subject of such importance, or at all interest those who have devoted themselves to researches of this description, it will fully repay me for the labour of transcribing it.

I remain, dear Sir,
Your's very faithfully,
CHARLES SYLVESTER.

Derby, Nov. 16th, 1812.

I. Arsenic.

White oxide of arsenic.

As a poison the most virulent, and, at the same time, the one most to be dreaded, from its extreme insipidity, and the consequent readiness with which it may become an instrument in the hands of the murderer, or be received into the system by accident, is the white oxide of arsenic. For the detection of this substance, when thus admitted, various processes have, at different times, been recommended; and the papers of Dr. Bostock, published in the 5th vol. of the Edinburgh Medical and Surgical Journal, have, very generally, been thought to contain the best directions for this purpose. After various experiments he decides in favour of sulphate of copper, and carbonate of potash, which precipitate the white arsenic under the form of *Scheele's green.* It cannot fail to have occurred, however, to every one who has repeated these experiments, that the phenomena produced in this process are very much too ambiguous to enable a man, where the life of an individual is at stake,

Detection of it.

Dr. Bostock's method generally preferred.

Objection to it.

stake, to swear positively, that arsenic has been detected in his operations. The alkali employed, whether arsenic be present or not, uniformly occasions a precipitate, by detaching the oxide of copper from its combination with the sulphuric acid. The colour of the deposit, it is true, is not absolutely the same in both cases; but, when it is recollected, that experiments of this kind are, for the most part, conducted on solutions coloured to a greater or less degree from the matters found in the stomach or intestines, this minuteness of distinction will be deemed scarcely appreciable by the eye even of the most experienced operator. Such uncertainty ought, most assuredly, not to attend investigations of this nature; and the process of Dr. Bostock, therefore, becomes objectionable from this circumstance.

Process lately recommended by Mr. Hume.

The method of detecting arsenic next deserving of remark, is one lately recommended by Mr. Hume, of London. It consists in adding a quantity of subcarbonate of soda to a solution suspected to contain this metal; and afterward presenting to it a small piece of nitrate of silver. It is far preferable, Mr. Hume finds, to employ the latter salt in a solid form; and he recommends an angular piece, of the size of a pin's head, or thereabouts, held at the surface of the fluid, on the point of a knife. The existence of arsenic will be shown by a yellow precipitate, which falls down in rather abundant quantity. Whenever arsenical mixtures are operated upon, which have but little contamination with foreign ingredients, this process will, undoubtedly, succeed very well; but if ever muriatic acid be present, and this is always the case where materials from the stomach are mixed with the fluids under experiment, the test is then wholly useless, as a muriate of silver will be immediately formed, and the yellow compound, said to be so unequivocal in its indication of arsenic, of course be prevented from appearing. Neither of the processes yet spoken of, therefore, can receive that confidence, which ought to attach to investigations of such high importance. This is not a mere speculative difficulty, but was fully proved to me during a course of experiments made some time ago, in consequence of a case of poison which came under my notice; and having, with the assistance of my friend, Mr. James Oakes, devoted, at that period, a good deal of attention to the subject, with a view, if possible, to supply the deficiency, we were led to the use of

Objection to it.

The difficulties proved by experience.

A different

process recommended.

Two tests proposed.

Advantages in the use of these tests.

Of the two, the acetate of copper preferable.

Preparation of the oxyacetate of iron

two reagents, which, I think, will not only be found free from the objections applicable to those already mentioned, but appeared to combine most of the advantages requisite in operations of such extreme delicacy. The reagents employed were the acetate of copper, and oxyacetate of iron. For the preparation of them it is merely necessary to decompose oxysulphate of iron, and sulphate of copper, by acetate of lead, adding the latter until a turbidness ceases to appear. The resulting mixtures should contain as little of the original ingredients in combination as possible; particularly the iron test, since an excess of the oxysulphate, as was observed in our experiments, suspends the action of the acetate, and prevents its combination with the arsenic. The presence of acetate of lead is objectionable from its causing a precipitate with arsenic, which cannot be distinguished by the eye from sulphate of lead. When the two acetates are properly prepared, they combine with arsenic with considerable facility; that of iron producing a bright orange yellow deposit, and that of copper, green. The decomposition, however, especially of the former, does not appear to be complete till they have been suffered to stand a few seconds. One of the great advantages attending these reagents is, that their action is independent of the use of alkali, which, in the two former processes, from the precipitate uniformly occasioned by its presence, throws considerable uncertainty over the results of an experiment; and where the mixtures are coloured, as will always be the case, in a greater or less degree, in examining the contents of the stomach, must rob these methods of the whole of their value. With the tests here recommended, the *colour* of the compounds produced is not of that primary importance; for, since almost all their combinations, particularly those of copper, are soluble in water, except the one produced by an union with the white oxide of arsenic, the appearance of any precipitate may, without much risk, be referred to the presence of this metal. Of the two, experience has confirmed us in a preference of the acetate of copper, partly from its more sensible action on arsenical mixtures, and in some measure, also, from the easier mode of its preparation. As the oxyacetate of iron, however, may sometimes be occasionally resorted to, in order to afford additional evidence of the accuracy of an experiment, it may be necessary to add, as a farther direction for its preparation, that the oxysulphate from which it is obtained, should be made by dissolving iron *with the aid of heat* in nitric acid, afterward precipitating the oxide, and redissolving it in sulphuric acid. The salt thus formed contains the metal at a *maximum* of oxidation.

Reduction of the arsenic to be preferred when practicable.

The whole of the above processes for the detection of arsenic of course refer to the cases where it has been exhibited only in a fluid state. Whenever it can be accomplished, however, by far the most satisfactory means of arriving at a knowledge of the presence of this substance, is to reduce it to a metallic state, which may be readily effected, either by subliming it in a glass tube with the aid of charcoal, or exposed between two plates of copper, according to the plans recommended in chemical works.

II. Corrosive Sublimate.

Oxymuriate of mercury.

For the discovery of corrosive sublimate, the methods almost exclusively resorted to until very lately were its precipitation by means of one of the carbonated fixed alkalis, or by lime water, which detach it under the form of an orange-coloured, or orange yellow, sediment. Dr. Bostock has since recommended muriate of tin; but, to the use of this test there is considerable objection, inasmuch as a precipitate, very similar in appearance to the one obtained from mercury, is always occasioned whenever muriate of tin comes into contact with a solution containing water. This could not fail to render the result of any experiment ambiguous; but should it so happen, that, from a particular circumstance, the employment of the muriate might be rendered at all desirable, its effect upon the fluid suspected to contain corrosive sublimate should be collated with the appearance produced from its mixture with an equal quantity of water, since the precipitate occasioned in a mercurial solution is remarkably more abundant than in the latter case, and sufficient to dispel all uncertainty arising from this cause. But a test, at once the most easy of application and satisfactory, is furnished by means of galvanism, in which the mercury is separated in a metallic state. This experiment can be made by any person, and almost in any situation. It is merely necessary to take a piece of zinc wire, or in its absence a piece of iron

Muriate of tin recommended by Dr. Bostock.

Objection.

This, in some degree, surmounted.

Galvanism supplies a much better taste.

iron wire, about three inches in length, bent twice at right angles into a shape something like the letter U, but with a flattened bottom*. Its width should be about equal to the diameter of a common gold wedding ring; and the two ends of the bent wire must afterward be tied to a ring of this description. This being accomplished, take a plate of glass not less than three inches square, lay it as nearly horizontal as possible, and on one side drop some sulphuric acid, diluted with about six times its weight of water, till it spreads to the size of a halfpenny. At a little distance from this, towards the other side, next drop some of the solution supposed to contain corrosive sublimate, till the edges of the two liquids join together, After this is done, let the wire and ring, prepared as above, be laid in such a way, that the wire may touch the diluted acid, while the gold ring is in contact with the suspected liquid. If the most minute quantity of corrosive sublimate be present, the ring, in a few minutes, will be covered with mercury on the part which touched the fluid. It might be proper to filter the mixture before submitting it to experiment, or otherwise to pour it clear from the top; since calomel, which is so frequently taken as a medicine, might possibly be present, and give rise to these appearances. The insolubility of this substance, however, enables us easily to avoid it by the precautions here suggested.

III. Lead.

Lead.

Although lead is not so virulent a poison as either arsenic or corrosive sublimate, its effects upon the animal economy are so greatly to be dreaded, that those liable to its influence in manufactures or domestic life, cannot be too much cautioned against it. The use of lead, in the construction of water cisterns, pumps, and conduit pipes, would, at first thought, appear highly objectionable; and in many instances it is, no doubt, productive of injury. The reason of its not being uniformly so, has been ingeniously pointed out by Gayton de Morveau. Most mineral waters contain a greater or less quantity of some salt formed by sulphuric acid. This acid is not only

Cautions against its use.

Why not uniformly injurious in these cases.

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* The Greek Π is no doubt the figure intended.

the means of precipitating any lead which may happen to be dissolved in the water, but has the effect also of completely coating the interior surface of the vessels with the sulphate thus formed, which is a substance not liable to decomposition, and therefore defends the lead from all future action of any solvent in the mineral water.

Devonshire colic.

The dreadful disease called the *Devonshire colic* was clearly shown by Sir George Baker to be occasioned by the lead constituting the lining of the cider press, and other vessels, and which was dissolved by the acetic acid developed during fermentation. The acetic acid is here asserted to be the solvent, because the malic acid forms a salt with lead which is insoluble.

Great injury done by wine merchants.

The effects of this metal have been still more conspicuous in its use by wine merchants to correct the acidity of wine. The practice was at one time so common in France, that in a particular year, when much sour wine prevailed, many thousands of people are said to have fallen victims to its influence; and had it not been for the interference of government, it is impossible to say how widely this dreadful evil might have extended itself.

Lead present in new rum.

In the new rum of our West-India colonies the presence of lead has been marked by still more deadly consequences. It became a matter of great surprise, however, that, after this liquor had been kept in casks for twelve months, it lost its deleterious qualities. The lead employed in the vessels for the manufacture, but more especially in those for the distillation of rum, could not fail to introduce this metal in great quantity through the medium of the acetic acid, which is a constant product of fermentation; and had it not been for a circumstance about to be mentioned, it is difficult to conceive where the calamity might have terminated. Nature, however, in the shape of accident, stepped in as mediator, and redeemed the lives of those destined to drink the fascinating potion. It was before observed, that the rum lost its poisonous property by remaining a certain time in the casks; yet, although the fact was known, and the evil remedied, many years ago, I am not aware that any one has accounted for the change produced in the liquor. About two years since, my friend, Dr. Forester, of this place, gave an interesting lecture, on behalf of the Literary and Philosophical Society, upon the subject of poisons; and

How this is remedied by keeping.

and it was not till then, that I became acquainted with the curious facts above mentioned. It immediately occurred to me, that gallate of lead was insoluble; and I lost no time in making some experiments, to ascertain the fact. The method by which I prepared some gallic acid for the purpose may, perhaps, be new, and not wholly uninteresting, to some of your readers. My first step was to make a strong tincture of nutgalls in proof spirit. To this was added, by little at a time, a nearly saturated solution of isinglass, till the whole of the tannin was precipitated. The liquor separated from the coagulum at first appeared rather opaque, but without colour. By standing at rest for a few days, a deposition of flocculent matter took place, consisting of gelatine and tannin, which left the liquor transparent and colourless. This I considered as a solution of gallic acid, nearly pure. At all events, it did not contain any substance which prevented its being an excellent test for iron or lead. I soon found, that in very dilute solutions of lead, where sulphuric acid, or a sulphate, produces no visible precipitate, the presence of this metal was made sensible by the aid of the gallic acid. This confirmed my suspicions on the subject, and left me in no doubt as to the real cause of the rum losing its pernicious qualities; for, since the joint existence of lead and gallic acid in any fluid is impossible, from the formation of an insoluble gallate, the lead of the rasa becomes precipitated by the gallic acid furnished by the oak cask. These facts supply an excellent, though indirect, method of ascertaining, in many instances, whether lead be dissolved in wines. If, on testing the wine with iron, it is found to contain gallic acid, we may safely infer, that no lead is present; but if no gallic acid be detected, then either this acid, or the sulphuric, may be added, which will precipitate the lead in the form of a white powder. Sulphuret of potash, or lime, may also be employed, which will occasion a blackish deposit.

Mode of preparing gallic acid.

A sensible test of lead.

Means of detecting lead in wine.

The prevailing use of lead in dairies is very objectionable, especially when the milk is immediately used as an article of food. On the separation of the curd and butter, the dissolved lead will, no doubt, exist in the whey. When milk is kept too long in warm weather, the acetic acid is formed, which takes up the lead; and it is a fact well known in dairies, that milk remains sweet longer in leaden vessels than any other. This is,

Lead should not be used in dairies.

is, mistakenly, attributed to the *coolness* of the lead; but the true cause is as above mentioned. The evil would, in all probability, be much more considerable, were it not for the presence of the saccholactic acid, which takes the lead from the acetic acid, and forms an insoluble compound.

IV. Copper.

Copper.

Ammonia its proper test.

The only other metallic substance likely to be taken into the stomach is copper; and for this the beautiful blue colour produced in its solutions by pure ammonia, is the most decisive and satisfactory evidence that can be required.

General test of metallic poisons.

General precautions.

As a general reagent, either for the present metal, or for lead, mercury, or arsenic, none, in point of delicacy, can exceed liquid sulphuretted hidrogen. It detects the smallest quantity of metallic matter present in any mixture; and although the coloured media, in which experiments of this sort are generally obliged to be made, prevent that reliance upon the mere colour of a precipitate requisite to give this test an exclusive preference; yet it may frequently abridge the labour of an operation very considerably, and at once decide whether the poison has been metallic or otherwise. In conducting this sort of experiments, the recommendation of Dr Bostock, to view the result by *reflected*, and not by *transmitted*, light, is highly important; and in no case, perhaps, ought a decision to be given without comparing the effect of every test on the suspected mixture with the phenomena it presents in fluids of known composition.

XII.

On facilitating the Emission of Roots from Layers. *By* T. A. KNIGHT, *Esq. Pres. H. S**.

Experiments on the generation and motion of sap in trees.

IT is my custom, annually, to repeat every experiment that occurs to me, from which I have reason to expect information either in opposition, or in favour, of the opinions I have advanced respecting the generation and motion of the sap in trees; and one of these experiments appearing to point out an improvement in the propagation of such trees by laying, as do not readily emit roots by that process, I send the following statement, under the hope that it may be acceptable to the Horticultural Society,

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* Trans. of the Hort. Soc. vol. I. p. 255.

Sap descends from the leaves through the bark.

Proof of this.

I have cited, in a former communication*, a part of the evidence, upon which I have inferred, that the sap of trees descends from their leaves through the bark; and I shall here only observe, in support of this opinion, that, if a piece of bark be every where detached from the tree, except at its upper end, it will deposit, under proper management, as much, or nearly as much wood, upon its interior surface, as it will if it retain its natural position; and that the sap which generates the wood, deposited in the preceding circumstances, must descend through the bark, as it cannot be derived from any other source.

Sap employed in the formation of new roots in a layer.

When a layer is prepared, and deposited in the ground, the progress of the sap, in its descent towards the original roots, is intercepted upon the side where the partially detached part, or tongue, of the layer is divided from the branch; and this intercepted sap is, in consequence, generally soon employed in the formation of new roots. But there are many species of trees, which do not readily emit roots by this mode of treatment; and I suspected that, wherever roots are not emitted by layers, the sap, which descends from the leaves, must escape almost wholly through the remaining portion of bark, which connects the layer with the parent plant. I therefore attempted, in the last and preceding spring, to accelerate the emission of roots by layers of trees of different species, which do not readily emit roots, by the following means, having detached the tongue of the layers from the branches in the usual manner.

In layers where new roots had not been formed,

Soon after midsummer, when the leaves upon the layers had acquired their full growth, and were, according to my hypothesis, in the act of generating the true sap of the plant, the layers were taken out of the soil; and I found, that those of several species of trees did not indicate any disposition to generate roots, a small portion of cellular bark only having

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[*] Horticultural Transactions of 1811; Journal, vol. XXXII. p. 350.

issued from the interior surface of the bark in the wounded parts. I therefore took measures to prevent the return of the sap through the bark, from the layers to the parent trees, by making, round each branch, two circular incisions through the bark, immediately above the space where the tongue of the layer had been detached; and the bark, between these incisions, which were about twice the diameter of the branch apart, was taken off. The surface of the decorticated spaces was then scraped with a knife, to prevent the reproduction of the bark, and the layers were recommitted to the soil; and at the end of a month I had the pleasure to observe that roots had been abundantly emitted by every one. In other instances I obtained the same results, by simply scraping off, at the same season, a portion of the bark, immediately at the base of the tongue of the layers, without taking them out of the ground.

the return of the sap to the tree was prevented by removal of bark,

and roots were produced.

By the preceding mode of management, the ascending fluid is permitted to pass freely into the layer to promote its growth, and to return till the period arrives at which layers generally begin to emit roots; the return of the sap through the bark is then interrupted, and roots are, in consequence, emitted; and I entertain little doubt that good plants of trees, of almost every species, may be thus obtained at the end of a single season. I wish it, however, to be understood, that my experiments have been confined to comparatively few species of trees; and that I am not much in the habit of cultivating trees of difficult propagation.

Effect of this management.

XIII.

On the Cultivation of the Jamrosade (Eugenia Jambos L.) in the National Garden at Paris. Abridged from the account given by M. Thouin, *in the Annales du Museum, V.* 1, *p.* 357. *By* Richard Anthony Salisbury, *Esq. F. R. S. &c.*[*]

THE *jamrosade,* or *eugenia jambos* of Linné, is one of those trees, the fruit of which is seldom brought to perfection in *Europe.*—In *Hindostan,* where it grows wild, it is called *jambos,*

Jamrosade, or rose apple.

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[*] Horticult. Trans. vol. 1, Appendix, p. 11.

or *jambose*, and in those colonies where it is cultivated, *jamrosade*, or *rose apple*. There are several varieties, differing in the size and colour of their fruit; some red, or reddish; others white and smaller. *Rumph* calls the last variety *Jambosa Sylvestris alba*, and this is the tree I now propose to describe.

The species being already well known and figured, I shall only mention the differences peculiar to this variety with white fruit, its habit at *Paris*, and the method there adopted to make it produce fruit.

Described.

Our tree is at present about 11 feet high, with a stem 2 inches and a half in diameter at the base, branching from below the middle into a pyramidal head. The leaves are undivided, smooth, opposite, of a deep green, coriaceous, and not unlike those of some *Peach trees*, but larger. The buds push forth young leaves in the beginning of summer, of a most lively red, which change gradually to their permanent deep green colour. The bunches of flowers also appear at this period, terminating the branches, from 2 to 6 being clustered together. Petals, 4, greenish white, about as large as those of *apple* blossoms. Stamens very numerous, in a tuft half as long as the petals, their filaments pale violet colour towards the top, where they diverge, their anthers yellow. Pistil, longer than all, is inserted like the stamens, petals, and 4 divisions of the calyx upon a globular germen, which swells into a green fruit, gradually changing to white with a pale rose coloured tinge on the side exposed to the sun.

The fruit.

In size and shape, the fruit is not very unlike a *medler*: its flesh rather firm, but easily broken, from 2 to 3 lines thick, slightly acid, and perfumed with a smell approaching that of the rose, from which it has acquired the name of *rose apple* in some of the French colonies; in the middle are several nuts, easily detached from the flesh; if there is only a single nut, it is spherical, but when more are perfected, as is often the case, they become angular in the parts which touch each other. The shell of the nut is thin and brittle, inclosing a greenish white kernel, which easily breaks into irregular pieces. The cavity of the kernel, varying in size and figure, but more or less oval, is lined with a brown pellicle, which adheres very slightly. These fruits ripen from *September* till *December*, and though not actually nutritious,

tious, their perfumed flavour renders them very agreeable to most palates.

Its introduction into France.

The individual one above described was brought from *Hindostan* in 1765, by the abbé Gallois, and placed in the late Mr. Lemonnier's stove at *Versailles*. Though very young, by plunging it in the tan-bed, it soon flowered, but never ripened fruit till 1786. When it had attained the height of 6 feet, it was transplanted into a small box, and exposed gradually to the open air, during two of the hottest months of the year, but afterward removed back to the tan-bed.

Attempt to treat it more hardily.

In 1794 this tree was added to the National collection, and being stout and vigorous, I determined to treat it more hardily. During winter, instead of the tan-bed, it stood on the floor of the stove, but near the flue, and during summer it was exposed to the open air, in a sheltered southern exposure, not housing it till *October*. This method of culture, however, did not agree with it; for, soon after being put out, most of the leaves fell off, and those which remained, as well as the ends of its branches, turned yellow; a plain indication of its sufferings from the cold nights. Nevertheless, the great heat of our *Paris* summer soon restoring it to its ordinary vigour, numerous young shoots, and many flowers pushed out, but they fell off without producing fruit. In this way, I persisted to cultivate this tree till last spring (1801), being anxious to try, if in so many years, it might not be habituated to our climate; but it annually underwent the same alteration of sickness and health already detailed*.

Treatment to make it produce fruit.

At this period, wishing to make it produce fruit, I thought all that might be necessary would be a large portion of air with very great heat. For this purpose, it was left in the great stove at the foot of a very white wall, which, by reflecting the rays of the sun, increased the heat still more, and the tree was so placed as to receive the rays perpendicularly. The air was suffered to blow freely round it, and it was deluged with water, in consequence of the great evaporation produced by so much heat and air.

Successful.

My wishes were thus completely fulfilled; the tree grew most luxuriantly, being covered in *June* with numerous flowers, which were rapidly fecundated, the greater part of them being succeeded by ripe fruits, of which I gathered more than 40. Some of the finest are preserved in the gallery of Natural History; of others, which fell off, I have already sown the seeds: and others still on the tree will be suffered to remain till they drop off spontaneously, that I may be quite certain their seeds are perfectly ripe. From an examination of the kernel, which soon changes to a hard, horny substance, it is not surprising, that all the seeds imported from abroad have hitherto failed, unless they have been sent packed in earth; and I therefore deemed it necessary to sow them in a few days after they fell from the tree.

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* This account does as great honour to the candour of one of the first gardeners in the world, as his detail of the insertion of the several parts of the flower does to his botanical abilities.—*Sec.*

Seeds sown.

Treatment of the seeds.

To make success in this point doubly sure, I employed a method, the good effects of which I have often experienced. This was, after taking the nuts out of the fruit, to put them in my breeches pocket for 2 or 3 days. This sort of animal bath is preferable to the custom which has hitherto prevailed of immersing many seeds of hot climates in pure water.

Mode of sowing.

I finally sowed these nuts about half an inch deep in pots of earth, plunged in a very gentle hot bed. At the approach of frost they will be removed to the tan-bed of the stove, when the essential point to attend to, will be to moderate the humidity, heat, and light, so that the young plants may not appear till spring.

The plants may probably thrive in a temperate stove.

I dare not hope that this tree will soon be naturalized to live in the open air in any part of *France*; for, its buds (gemmæ) have no scales; but we may reasonably expect, that the plants raised from seeds here will not be so delicate as imported plants, and that they may succeed in a temperate stove, or orangery: nay, it is even possible, that such plants may survive through winter in some of the warm spots under our southern maritime alps, or in the island of *Corsica*. For this purpose, they should be planted with *orange* trees, *citron* trees, and *guava* trees, among which the *jamrosade* thrives in its native country, or such colonies as it has been transported to.

XIV.

Letter from Dr. Tuthill on the Sugar from Potato Starch.

To Mr. Nicholson.

DEAR SIR,

HAVING learned, that professor Berzelius had brought intelligence to this country of a very remarkable change produced in wheat starch by the action of dilute sulphuric acid at a high temperature, as discovered by M. Kirchoff, of the imperial academy of St. Petersburgh, I was desirous of ascertaining whether the fæcula of other vegetables, submitted to the action of the same fluid, at the same temperature, would exhibit a similar phenomenon. For this purpose I took eight pounds and three quarters of potatoes, grated them, and placed the pulp on a sieve. Cold water was then slowly poured upon this pulp as long as it passed turbid through the sieve, and the liquor was suffered to stand in the vessel that received it till it became clear. On pouring off the clear liquor, the fæcula of the potatoes was found at the bottom of the vessel; and, when dried by a very gentle heat, weighed a pound and a half. To this fæcula were added six pints of distilled water, and a quarter of an ounce by weight of common sulphuric acid in an earthen vessel furnished with a cover. The mixture was kept boiling for thirty-four hours without intermission, the vessel being covered, and the loss by evaporation carefully supplied by the frequent addition of distilled water, so as to preserve the same quantity as at the commencement of the operation. For the first twelve hours I could perceive no change in the sensible properties of the vapour. At the expiration of twenty-four hours the liquor had evidently become saccharine, and this quality continued to increase as the boiling was prolonged. Thirty-four hours after the commencement of the ebullition, half an ounce of finely-powdered charcoal was added, and the boiling continued for two hours longer. The acid was then saturated by lime that had been very recently burned, and the boiling continued for half an hour; after which the liquor was passed through a piece of calico, and the substance remaining on the filter washed by the repeated effusion of warm water. This substance, when dry, weighed seven eighths of an ounce, and consisted of charcoal and sulphate of lime. The clear liquor was now evaporated in a water bath to the consistence of sirup,

Invention of sugar from starch.

Potato starch.

A pound and a half of starch, and six pints of water, and one quarter of an ounce of sulphuric acid were gently boiled thirty-four hours.

In twenty-four hours the fluid was saccharine.

After 34 hours an ounce and a half of charcoal was added.

After two hours more the acid was saturated by lime.

Insoluble precipitate.

Clear liquor evaporated.

Afforded sugar equal to one seventh part of the weight of the potatoes.

rup, and set aside to crystallise. In eight days it was converted into a crystalline mass, having nearly the sensible properties of common brown sugar mixed with a little treacle. The weight of the saccharine matter thus obtained from eight pounds and three quarters of potatoes, and which I conceive to be intermediate between cane sugar and grape sugar, weighed one pound and a quarter.

One pound of this crystallised saccharine matter was now redissolved in four pounds of distilled water, and by the addition of a quarter of an ounce of yeast submitted to alcoholic fermentation. In ten days the temperature having varied from 44° to 54°, the smell of the liquor first indicated that the alcoholic fermentation was just beginning to pass into the acetous. The whole was then instantly submitted to distillation, and the process continued till a pint and a half of fluid was collected in the receiver. This on being redistilled produced two ounces and five eighths by measure of dilute alcohol, of which a cubic inch, the mercury in Fahrenheit's thermometer standing at 45° and in the barometer at 22$\frac{19}{20}$ inches, weighed 245 grains. I have therefore concluded from the accurate experiments of Sir Charles Blagden, that the two ounces and five eighths of dilute alcohol thus obtained contains fourteen drachms by measure of proof spirit.

I am, dear Sir, very truly your's,
G. L. TUTHILL.

October 28, 1812.
Soho Square.

SCIENTIFIC NEWS.

New Explosive Compound.

Notice has been received from the Continent of a new explosive compound, upon which Sir Humphrey Davy has made some experiments; and it has also been produced by others. The present short statement is all that I can on this occasion insert.

Nitrate of ammonia is to be dissolved to saturation in water, and exposed in a basin to a low temperature, such as that of ice, or rather the freezing mixture of ice and salt.—A vessel containing oximuriatic gas is then inverted in the solution. The gas becomes absorbed, and the solution ascends; and, after one or two hours, a small portion of heavy oil is found at the bottom of the basin. Of this oil if a quantity of the size of a pin's head be put into contact with olive oil, a violent and dangerous explosion takes place.

A friend who repeated this experiment, used a four ounce phial of the gas and put his olive oil in a small platina spoon. The spoon was destroyed by the explosion.

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SUPPLEMENT TO VOL. XXXIII.

ARTICLE I.

Observations on the Measurement of three Degrees of the Meridian, conducted in England, by Lieut. Col. William Mudge. By Don JOSEPH RODRIGUEZ. *From the Philosophical Transactions for* 1812, *p.* 321.

Problem To ascertain the figure and magnitude of the earth

THE determination of the figure and magnitude of the earth has at all times excited the curiosity of mankind, and the history of the several attempts made by astronomers to solve this problem might be traced to the most remote antiquity. But the details of the methods pursued by the ancients on this subject being extremely vague, and their results expressed in measures of which we do not know the relation to our own, in fact give us very little assistance in learning either the figure or dimensions of our globe.

was considered by Huyghens and Newton,

It was not till the revival of science in Europe, that the two great philosophers, Huyghens and Newton, first engaged in the consideration of this question, and reduced to the known laws of mechanics, the principles on which the figure of the earth should be determined.

as determinable by attending to its rotation;

They demonstrated, that the rotatory motion should occasion differences in the force of gravity in different latitudes, and consequently, that parts of the earth in the neighbourhood of the equator should be more elevated than those near the poles.

which, on the hypothesis of the mass being uniformly dense, and imperfectly fluid, would elevate the equatorial more than the polar regions.

The most simple hypothesis, which first presented itself to their imagination, was that which supposed the earth to be throughout composed of the same kind of matter, and its surface that of a spheroid generated by revolution round its axis. This hypothesis, adopted by Newton only as an approximation to the truth, is, in fact, perfectly consistent with the equilibrium to which particles in a state of paste, or of tardy fluidity, would arrive in a short time after their present motion was impressed; and the eccentricity derived from this hypothesis is at least not very remote from that which actually obtains in the present state of consistence and stability which the earth has since acquired.

But geological observations show that the (external part of the) earth is not homogeneous,

But the homogeneity of the matter, of which the earth consists, is at variance with all geological observations, which prove evidently that at least 5000 toises of the exterior crust is formed of an immense mass of heterogeneous matters, varying in density from each other; and upon the supposition of a state of fluidity of the whole, it should follow, that the strata should successively increase in density from the surface towards the centre, that the more dense would accordingly be subjected to less of centrifugal force, and consequently that the spheroidical form resulting from this cause would be less eccentric than would arise from a state of perfect homogeneity.

Solution of the problem, by measuring several arcs of the meridians.

The most simple, as well as the most effectual means of verifying the hypothesis respecting the figure of the earth, is to measure in the two hemispheres several arcs of its meridians in different latitudes, at some distance from each other. On this subject it must be allowed, that the Academy of Sciences at Paris set the example, in giving the original impulse to the undertaking, and not only commenced, but put in execution those parts of the plan which were most difficult and most decisive.

The first measurements, as well as the vibrations of pendulums, showed that the polar regions are flattened-

The results of the first measurements made of different arcs on the meridian of different parts of the world, were found to be perfectly conformable to the expectations of Huyghens and of Newton, and also with experiments made on the vibration of the pendulum in different latitudes; and they left no doubt that the earth was in fact flattened at the poles;

establishing thereby one point extremely interesting in natural philosophy.

Inaccuracy of the earlier measures.

These results, however, did not correspond with sufficient accuracy for ascertaining with precision the degree of eccentricity, or even the general dimensions of the earth; as might naturally be expected, when we consider the necessary imperfection of the means then employed in these operations, and the great difficulties that are to be encountered.

For the purpose of making a nearer approximation to the true dimensions of the earth, and of verifying former measurements, it is necessary, in some instances, to repeat them, and also to make others in different situations, which may be expected to be improved in proportion to the progress that is made in the means of perfecting the several departments of science.

Grand undertaking of admeasurement in France and Spain:

At the commencement of the French revolution, men of science took advantage of the general impulse which the human mind received in favour of every species of innovation or change, and they proposed making a new measurement of an arc of the meridian in France, for the purpose of establishing a new system of weights and measures, which should be permanent, as being founded on the nature of things.

directed by a commission of the academy,

A commission, composed of some of the most distinguished members of the Academy of Sciences, was charged to form the plan of these operations, which were to serve as the basis of the new system. They invented new instruments, new methods, new formulæ; and in short almost the whole of this important undertaking consisted of something new in science.

and performed by Delambre and Mechain.

Two celebrated astronomers, Delambre and Mechain, were engaged to perform the astronomical and geodetical observations, and these they continued as far as Barcelona in Spain. The details of their operations, observations, and calculations, were subsequently examined by a committee of men of science, many of whom were foreigners collected at Paris, who confirmed their results, and by the sanction of such an union of talents, gave such a degree of credit and authenticity to their conclusions, as could scarcely be acquired by other means.

Continuation of the line by Biot and Arago to Formentera.

Since that time, in the year 1806, Messrs. Biot and Arago, members of the National Institute, were sent into Spain for the express purpose of carrying on the same course of operations still farther southward, from Barcelona as far as Formentera, the southernmost of the Balearic islands. Fortunately this last undertaking, which forms a most satisfactory supplement to the former, was completed by the month of May, 1808, at a period when political circumstances would not admit of any further operations being pursued, as a means of verifying the results, by measuring a base which should be independent of those formerly obtained in France.

Verification of the Lapland measures by the Swedish academy.

In the year 1801, the Swedish Academy of Sciences, encouraged by the success of the operations conducted in France, sent also three of its members into Lapland, to verify their former measurement, taken in 1736, by new methods, and by the use of new instruments, similar to those which had recently been used in France, and of which the National Institute made a handsome present to the Swedish Academy. The results of this new undertaking, which terminated in 1803, were drawn up by M. Svanberg, and are highly interesting, by their exactness, by the perspicuity of the details, and even a certain degree of novelty given to the subject by the arrangement adopted by the learned author M. Svanberg.

The agreement of these new measures in confirming the general results

These new measures were found to confirm, in a remarkable manner, the general results of those which had preceded, and gave very nearly the same proportion for the eccentricity and other dimensions of the globe, so that there would not have remained the smallest doubt respecting the figure of the earth being flattened at the poles, had there not been a fourth measurement performed in England at the same time as that undertaken in Lapland, the results of which were entirely reverse. This measurement, which comprised an arc of 2° 50′, was undertaken by Lieut. Col. Mudge, Fellow of the Royal Society, with instruments of the most perfect construction that had ever yet been finished by any artist, contrived and executed for that express purpose, by the celebrated Ramsden. The details of the observations and other operations of Lieut. Col. Mudge, may be seen in the volume of the Philosophical Transactions for the year 1803; and one cannot but admire the beauty and perfection of the instruments employed by that skilful observer, as well as the scrupulous care bestowed on every part of the service in which he was engaged. Bengal lights were employed on this occasion, as objects at the several stations,

was contradicted by the results of the admeasurements made in England

under circumstances of pestations, and their position appears to have been determined with the utmost precision by the theodolite of Ramsden, which reduces all angles to the plane of the horizon, and with such a degree of correctness, that the error in the sum of the three angles of any triangle, is scarcely, in any instance, found to exceed three seconds of a degree, and in general not more than a small fraction of a second.

culiar advantage:

Accordingly the geodetical observations were conducted with a degree of exactness, which hardly can be exceeded; and even if we suppose for a moment, that the chains made use of in the measurement of the bases, may not admit of equal precision with the rods of platina employed in France, nevertheless, the degree of care employed in their construction, in the mode of using them, and the pains taken to verify their measures was such, that no error that can have occurred in the length of the base, could make any perceptible difference in the sides of the series of triangles, of which the whole extent does not amount to so much as three degrees.

Nevertheless, the results deduced by the author, from this measure alone, would lead to the supposition, that the earth, instead of being flattened at the poles, is, in fact, more elevated at that part than at the equator, or at least, that its surface is not that of a regular solid. For the measures of different degrees on the meridian, as reduced by Lieut. Col. Mudge, increase progressively toward the equator.

but which indicate, that the earth is more elevated at the poles than at the equator.

The following table of the different measures of a degree in fathoms, is given by the author in his Memoir.

Table of the measures of a degree of lat. in England, which *increase in going southward.*

Latitude.	
52° 50′ 30″	60766
52 38 56	60769
52 28 6	60794
52 2 20	60820
51 51 4	60849
51 25 18	60864
51 13 18	60890
51 2 54	60884

The singularity of these results excites a suspicion of some incorrectness in the observations themselves, or in the method of calculating from them. The author has not informed us in

This singular result,

his Memoir, what were the formulæ which he employed in the computations of the meridian; but one sees, by the arrangement of his materials, that he made use of the method of the perpendiculars without regard to the convergence of the meridians; and although this method is not rigorously exact, it can make but a very few fathoms more in the total arc, and will have very little effect on the magnitude of each degree. It is therefore a more probable supposition, that, if any errors exist, they have occurred in the astronomical observations. But it is scarcely possible to determine the amount of the errors, or in what part of the arc they may have occurred, excepting by direct and rigorous computation of the geodetical measurement. I have therefore been obliged to have recourse to calculations, which I have conducted according to the method and formulæ invented and published by M. Delambre.

if erroneous, is most probably so from the celestial observations.

The means generally employed for finding the extent of a degree of the meridian, consists in dividing the length of the total arc in fathoms, by the number of degrees and parts of a degree deduced from observations of the stars; but if these observations are affected by any error, arising from unsteadiness of the instrument, from partial attractions, or from any other accidental causes, then the degrees of the meridian will be affected, without a possibility of discovering such an error in this mode of operating. It is consequently necessary, in such a case, to employ some other method, which may serve as a means of verifying the observations themselves, of detecting their errors, if there be any, or at least of shewing their probable limits.

The usual method of finding the degree, by dividing the total arc in fath. by its measure in deg. and parts, will not detect the errors of observation.

My object therefore is to communicate the result of calculations that I have made, from the data published by Lieut. Col. Mudge in the Philosophical Transactions: and I hope to make it appear, that the magnitude of a degree of the meridian, corresponding to the mean latitude of the arc measured by this skilful observer, corresponds very exactly with the results of those other measurements that have been above noticed.

Calculations by the author,

In M. Delambre's method nothing is wanting but the spherical angles, that is to say, the horizontal angles observed, corrected for spherical error. Moreover, for our purpose, we have no occasion for the numerical value of the sides of the series of triangles, but only for their logarithms. Thus the logarithm

by means of the spherical angles in Delambre's method.

logarithm of the base measured at Clifton, as an arc gives us that of its sine in feet or in fathoms, so that by means of this latter logarithm, and the spherical angles of the series of triangles, we obtain at once, and as easily as in plane trigonometry, the logarithms of the sines of all their sides in fathoms.

After this, it is extremely easy to convert them into logarithms of chords or of arcs, for the purpose of applying them to the computation of the arcs on the meridian or azimuths. I give the preference to taking the logarithms of the sides as arcs, because the computations become in that case much more simple and expeditious.

Reduction of the northern base to toises.

Near to Clifton, which is the northern extremity of the arc, in a situation elevated 35 feet above the level of the sea, a base was measured of 26342,7 feet in length, the chains being supposed at the temperature of 62° Fahrenheit, or $13\frac{1}{3}$° Reaumur.

For reducing this base to toises, we have the proportion of the English foot to that of France, as 4 : 4,263, so that if p be taken to express the fractional part of the French foot, corresponding to English measure, then log. $p=9,97234,46587$,

and then log. of $26,342,7=4,42066,02860$,

and hence the log. of the base in toises will be found equal to 3,61485,36943, and the number of toises corresponding is 4119,5 taken at the same temperature, which corresponds to $16\frac{2}{3}$° of the centigrade thermometer.

This base we must consider as an arc of a circle, and it is easy to reduce it to the sine of the same arc, according to the method given in a note at the end of this memoir. The logarithm of the *sine* of the base in toises is found to be 3,61485,35800.

from which and the spherical triangles the portions or intervals of the stations with their azimuths were computed.

With this quantity as base, and by means of the spherical triangles given by Lieut. Col. Mudge in his paper, I have found the logarithmic sines in toises of all the sides of his series of triangles, and have subsequently reduced them to logarithmic arcs of the same, which enable me to complete the rest of the calculation. With these we may compute any portions of the meridian, or successive intervals of different stations expressed in toises, and in parts of the circle, or their respective azimuths, having regard always to the relative convergence of different meridians.

The author has made observations for determining the latitude of the two extremities of his arc, and has also determined the azimuths of the exterior sides in his series of triangles by means of the greatest elongation of the pole star.

In the calculations that I have made, I began at Clifton in Yorkshire, the northern extremity of the arc, and for this purpose the following are the data furnished by Lieut. Col. Mudge.

Col. Mudge's data,

Latitude of Clifton reduced to the centre of the station 53° 27′ 36,″62.

Azimuth of Gringley, seen from Clifton, and reckoned from the north toward the west 256° 17′ 25″.

Azimuth of Heathersedge seen from Clifton, and reckoned in the same direction 118° 8′ 8″,81.

from which the computations were begun,

With these data, and the two tables of spherical triangles and the logarithms of their sides expressed in arcs, the intervals between Clifton and the two stations Gringley and Heathersedge were found in toises and in seconds of a degree, as well as all the corrections to be made on the first azimuths increased by 180°, as azimuths of Clifton seen on the horizon at these latter places.

and continued through the whole series.

The same process was continued for the following stations in succession, all the way to Dunnose in the Isle of Wight, which is the southernmost extremity of the series.

In this manner we have the latitudes and azimuths of each station, by means of two or three preceding stations, and consequently we have a verification of all the calculations that have been before made by Lieut. Col. Mudge.

The results of my calculations are contained in the two following tables.

First Table of Distances in Toises and in Seconds of a Degree on the Meridian, comprised between the westerly Stations in the Series of Triangles.

Table of distances of the westerly stations on the Meridian;—in toises and in seconds.

Names of the Stations.	Arcs in toises.	Arcs in Seconds.
Clifton	0,0	0,0
Heathersedge	6834,324	430,9928
Orpit	15818,489	997,5928
Castlering	19801,1934	1248,8226
Corley	14295,384	901,6207
Epwell	22327,008	1408,2543
Stow	9555,479	602,7284
Whitehorse	18799,645	1185,8656
Highclere	14990,567	945,6354
Dean Hill	16105,614	1016,0180
Dunnose	23529,886	1484,4531
Sum total	162057,5437	10221,9837

Second Table of successive Intervals between the Eastern Stations.

Table of the intervals between the E. stations.

Names of the Stations.	Arcs in toises.	Arcs in Seconds.
Clifton	0,0	0,0
Gringley	2809,105	177,149
Sutton	10838,816	1061,931
Holland Hill	4681,190	295,2251
Bardon Hill	18092,261	1141,0462
Arbury Hill	27956,417	1763,2683
Brill	22374,106	1411,2769
Nuffield	14350,3834	905,2155
Bagshot	12137,933	765,6822
Hindhead	14449,2027	911,5140
Butser Hill	7853,644	495,4551
Dunnose	20514,036	1294,1974
Sum total	162057,0941	10221,9607

The arithmetic mean of those results gives the mean degree =57073 toises, of lat. of the whole arc.

Now if we take the arithmetic mean of the sums contained in the two tables, we have for measures of the entire arc, comprised between the stations of Clifton and Dunnose, the following quantities 162057,32 toises, and 10221,972 seconds of a degree, or 2° 50′ 21″,972. By dividing the former of these by the second, we get the measure of a degree, corresponding to the mean latitude of the whole arc, equal to

57073,74

57073,74 toises,, or 60826,34 fathoms, at the temperature of 16⅔° of the centigrade thermometer, the latitude being 52° 2′ 20″.

But by dividing the whole arc into two nearly equal parts, the northern mean deg. proves=57068,41 toises;

The station at Arbury Hill happens to be very nearly in the meridian of Clifton and Dunnose, and divides the interval between them into nearly equal parts. The measures of that part of the arc, which lies between Arbury and Dunnose, is by the tables 91679,47 toises, and 9783″,34 seconds, or 1° 36′ 23″,34 of the common division of the circle. The mean latitude of the arc is 51° 25′ 21″. And the measure of 1 degree corresponding to it is 57068,41 toises.

and the southern mean deg.=57080,70 toises.

In the same manner the measure of the arc comprised between Arbury Hill and the northern extremity at Clifton, is 70377,85 toises, and 4438,63 seconds, or 1° 13′ 58″,63. Its mean latitude is 52° 50′ 32″. And we have for one degree of the meridian, corresponding to this latitude, 57080,70 toises.

and the degrees increase in going northward,

Hence, if we divide the entire arc into two equal parts, we deduce the following values of a degree corresponding to the middle of the whole and of its parts.

Latitudes.	
51° 25′ 20″	57068
52 2 20	57074
52 50 30	57081

in perfect conformity with the theory and the results of other admeasurements.

These values are, as appears, perfectly in conformity with the theory, and with the results of other measures that have been taken in different parts of the northern hemisphere; but, in order to place that agreement in a more distinct point of view, I shall show how nearly these estimates agree with the elliptic hypothesis, by comparing them with those measures of a degree, on which we can place the greatest reliance for exactness.

Inquiry into the errors which led to the former conclusions.

Now, if we compare the results of these calculations with those deduced by Lieut. Col. Mudge from his observations, we shall see the probable source of those *errors*, which it appears to me have led him to false conclusions. It has already been observed, that the station at Arbury Hill divides the whole arc into two parts nearly equal, and that it is also nearly in the meridian of the two extremities at Dunnose and Clifton. It was, in all probability, this circumstance which determined the author to observe the latitude of Arbury Hill, as he would then have two partial arcs independent of the whole and of each other.

For determining the angular extent of these arcs, Lieut. Col. Mudge observed the zenith distances of several stars on the meridian above the pole, by means of a large zenith sector constructed by Ramsden, with the same pains that he had bestowed upon the theodolite. Lieut. Col. Mudge paid all possible attention, and took all such precautions as might naturally be expected from an observer of his experience and address. Nevertheless the results of his observations made on different stars, differ no less than 4 seconds from each other. But, by taking a mean of all, the dimensions of the three arcs reduced to the centre at each station are as follows.

Facts. The angular extent of the arcs were observed by Col. Mudge with a zenith sector;

in which the results vary 4 sec.

Between Clifton and Dunnose	2° 50′ 23″,35
Clifton and Arbury	1 14 3 ,40
Arbury and Dunnose	1 36 19 ,95

Means of observation in deg. and pts.

The extent of the first arc, in linear measure, is 1036339½ feet English, and when this is reduced to toises, we have for the lengths of the three arcs from Lieut. Col. Mudge's measures,

From Clifton to Dunnose	162067,3
Clifton to Arbury	70380,2
Arbury to Dunnose	91687,1

The same arcs in toises:

These last values exceed those resulting from my computations, the first by 10 toises, the second by 2, the third by 8 toises; and these differences arise from the convergence of the meridians, which the author thought might safely be neglected, and in fact it does not make a difference that is perceptible in the value of a degree upon the meridian. For the difference of 8 toises, in the distance between Dunnose and Arbury, makes but 5 toises difference in the value of a degree upon that arc, and the difference of 10 in the whole distance from Dunnose to Clifton, makes 3½ in the measure of each degree on that arc. So that, as far as this source of disagreement is concerned, the author's results and mine would not be found to differ materially from each other.

are not materially differing from the computed tables.

But, if we attend to the angular dimensions of the several arcs, as deduced from observations and from calculation, these will not be found to agree so nearly.

The following table will shew the differences in each instance.

But in deg. and pts they differ very considerably.

Clifton and Dunnose	2° 50′ 23″,35	observed
	2 50 21 ,97	calculated
Difference	+ 1 ,38	
Clifton and Arbury	1° 14′ 3″,40	observed
	1 13 58 ,63	calculated
Difference	+ 4 ,77	
Arbury and Dunnose	1° 36′ 19″,95	observed
	1 36 23 ,34	calculated
Difference	− 3 ,39	

These differences are really considerable, and are capable of producing important errors in the results dependent on them.

namely 40 toises in a degree.

In the first place we see, that the southernmost arc between Dunnose and Arbury is smaller than it would appear by computation, by as much as 3″,4, and when this deficiency is combined with an excess of 8 toises in the linear dimensions of the same arc, it makes as much as 40 toises difference in the estimated length of a degree. The reverse of this occurs in the northern portion of the arc comprised between Clifton and Arbury Hill. This is larger than it ought to be by 4″,77, and hence the value of a degree on the meridian turns out too small by about 62 toises in its linear dimensions. Fortunately however, the excess of the total arc is extremely small; as it does not exceed 1″,38, so as to make but 5 or 6 toises difference in the length of a degree observed on the meridian, and corresponding to the mean latitude of the arc examined.

The excess of the total arc is, however, much less

Hence the apparent increase in the degs. is to be ascribed to error of obs. of lat. at Arbury Hill.

From what has been above stated, it seems almost beyond a doubt that it is to errors in the observations of latitude, that the appearance of progressive augmentation of degrees towards the equator, as represented by Lieut. Col. Mudge in his paper, are to be ascribed, and that it is especially at the intermediate station at Arbury Hill, that the observations of the stars are erroneous nearly 5 seconds, notwithstanding the goodness of the nstruments, and the skill and care of the observer. But, before insist farther on this head, I will answer one objection that nay be made to the principles of the method that I have pursued n this Memoir.

Those astronomers, who have hitherto undertaken the measurement of degrees of the meridian, have deduced their measures by simply dividing the linear extent by the number of degrees and minutes found by observation of the fixed stars taken at the two extremities of the arc. This is indeed the most simple that can be adopted; and it has the advantage of being independent of the elliptic figure of the earth, especially in arcs of small extent. The elements dependent on this figure, are too uncertain to be employed in calculating the angular intervals in the short distances between successive stations, even as a means of verification, without risk of committing greater errors than those to which astronomical observations can be liable. Accordingly one cannot safely make any use of it in cases where great accuracy is required.

Objection. The elements dependent on the ellipticalfigure, are too uncertain to be employed in calculating the intervals, &c.

I must admit the justness of this objection, and must therefore shew the extent to which it really applies to the present subject.

In the first place, I may suppose, that in consequence of some fault in the instrument, with respect to vertical position, construction, or some accidental derangement, there is an error of some seconds in the observations of the fixed stars. How is this to be discovered? This is not to be done by comparing the value of a degree on the meridian, as deduced from these observations, with the results of other measurements in distant parts of the globe. For if we find that these degrees so taken do not agree in giving the same ellipsoid, we are not to attribute all the differences to irregularities of the earth, without supposing any error on the part of the observer, of his instrument, or of other means employed in his survey.

but, on the other hand, if diff. measures give diff. ellipsoids—it must not be admitted that the whole variation be ascribed to the fig. of the earth and none to the errors of obs.

But this, in fact, is what has generally been done. It must, however, be acknowledged, that the majority of observers have not been in fault, as they could do nothing better; but too much reliance has been placed on the goodness of their instruments, their means, and other circumstances. It is true that irregularities of the earth and local attractions may occasion considerable discrepancies which are even inevitable; but before we decide that these are the real source of disagreement, we ought carefully to ascertain that there are no others.

(*To be continued.*)

II.

On the Roots of Trees. By *Mrs.* AGNES IBBETSON.

To W. Nicholson, Esq.

SIR,

The difficulties attending giving the root.

IN my last letter, it was my endeavour to give as exact an account as possible of the increase of trees, both in length and breadth; that which they made in spring and autumn, and that which (nearly at the same time) enlarges the trunks. I shall now venture on a more difficult task, the delineation of the root, which I have long delayed; for whenever I was on the point of attempting it, I feared I was inadequate to the undertaking, and put it off another year, till further *dissection*, and a more *thorough knowledge*, should satisfy me that I was capable of giving an account that would please myself, and do justice to the great object of my pursuit. For many years together I have recurred to the subject, studying it with the most indefatigable industry, and seeking in nature only for information: but for the last six months the quantity of roots, both fresh and dry, that I have dissected, the innumerable cuttings that I have subjected to the solar microscope of the roots of different trees of every age and size—in short, the endeavours I have made to collect facts sufficient to prepare myself to give an exact account of the laws by which the root is regulated—the power which governs it in its *exterior*, as well as *interior* form—the parts which compose, and the mechanism which moves it, has at last given me courage sufficient to venture on my task; and if I do not thoroughly satisfy my readers, I shall still show many things *perfectly unknown*; and, at a future time, I shall hope to add circumstances that may render it more complete, and more worthy the *attention* of the *public*: at least I can promise, that I shall advance nothing but what *all* may ascertain the truth of, nor enter into any detail that may not be proved to be just and true, by those who will take the trouble of seeking, both in dissection and practical gardening, that knowledge, which constant labour and watching has procured me.

Explanation of the root of a tree.

The first thing that strikes the mind with astonishment in the dissection of roots, is that excessive motion to which they are subject; each fibre, and each sap-vessel must be capable of changing its place, and of creeping individually into another situation; and yet so admirably is the tout ensemble contrived to make but *one whole*, that it rarely differs from that *form* and *fold*, which is allotted to that species of tree. The root of a tree is that part which is the *foundation* of the sap-vessels. I have said, that each *sap-vessel* of the *stem* is joined about two inches above the earth, to *two sap-vessels* of the root; and so wholly and individually do they belong to each other, that they cannot be divided, without causing the destruction of both; the root may, indeed, sometimes shoot out another sap-vessel, I believe; but the stem-vessel cannot shoot (in this situation, and thus aggregated) another root. This vessel has its little branch, flower, and fruit proportioned to its size; it is impossible to know what each stem cylinder, with its accompanying root, will produce, because it cannot be traced higher in the tree than the trunk; but from the root to that part I have often followed it in one lengthening string. Monsieur de St. Aubert, (who is pursuing the same course of study as myself,) confirms what I have now written, by dissections published just after my opinions, in this respect, appeared in your Journal; and any one that *studies from dissections*, must, I trust, be of the same manner of thinking, the truth so plainly appears in them. To the root is added many radicles with all the mechanism necessary to collect and throw up the nourishment procured from the earth around. That every plant has the power, from all the variety of soils and decomposed matter, to select that which best suits its nature, and convey it to the bottom of the root, where all the juices meet, and are properly compounded and assimilated to the nature of the plant, is a certain truth: this general reservoir is found at the part where the root begins *to contract*, to form the sap-root. It is known by the quantity of alburnum laid up there. I have long been convinced, that alburnum is the congealed juice

Radicles, with their mechanism.

juice into which all the various nourishment of the earth turns; part forms into sap, part into the jelly of wood; for the sap is nothing but this juice in a dissolved state, and the wood nothing more than alburnum, having the wood and bastard vessels lengthening by degrees, and running through it. It is impossible, that any wood vessels can be formed by this juice, but the sap may easily be converted into that jelly-like substance, which forms the *alburnum*, and the rest of the process may be seen to pass under your eye in the solar microscope; that is, the bastard vessels may be seen to lengthen, and the sap-vessels to pierce through the softer substance, for the completion of the wood.

First part of the root.

The root may be divided into three parts; the first part shows the difference between the stem and root, the latter having *double* the *number* of wood vessels, and no pith; for after the first three or four years, the pith always disappears in the root of trees, and the line of life occupies the *centre* in its stead; indeed, as the chief use of the pith is to moisten the wood vessels, that they may bend in every direction, and thus facilitate the exit of the buds; and as the roots of trees have few buds after that time to throw from the root, the pith would no longer be of use in the centre; the bark and inner bark are nearly the same as in the stem, and the row of alburnum rather larger—that the wood should be *double* in the root, to what it is in the stem—and that it should increase according to the increasing branch, is the most absolute proof that the sap flows in the wood, since no other part would produce nourishment sufficient to support the tree. But no person who dissects trees, can doubt this truth, as the immense sap-vessels, and their being loaded *with sap*, must carry conviction to the most incredulous, provided they see it properly magnified.

No pith after the third year.

Second part of the root.

The second part of the root is that which appears to be the reservoir. It has all the parts already mentioned, except that the bark is narrower, and that the part usually occupied by the alburnum, has from three to five rows of that matter, instead of one; they are wide and juicy, and the quantity most irregular. I have often seen them *almost heaped together*, forming at once from five to seven rows; but I never saw more. (See fig. 1. Pl. 8.) The alburnum is loose and thin, and far more watery and unfinished in its appearance, than in the stem, or first root. The tap root is the third division—it is of the utmost consequence to attend to the shoots that belong to the different roots—it is the tap root which always forms the leading shoot of the tree; and if it is cut, it will, without doubt, spoil that part, by forming *two middle stems* to the tree; at least I have generally found this to be the case; and as the beauty of a tree depends much on the perpendicular height of its single pillar, the custom they have in most nurseries of curtailing the tap root, is a most vicious one. A row of alburnum is seldom found in this part of the root; for it increases this way but once in seven or eight years—its growth is, indeed, in a different manner, shooting from the end; for if I sever the smallest piece from the tap-root, it will very soon throw out two ends, and if these are cut, two more will be added to each, and it then ceases to shoot perpendicularly; losing its form, and then growing like a common root, whereas a *tap-root* draws out at the end like a telescope, one inch each shoot; and if it is dissected *with care*, two or three of these divisions will be found. What is the use of the tap-root? By shooting perpendicularly down, to fix the tree *firmly* in the ground, and keep it straight in that position; then it is surrounded by radicles which perpetually pump up from every different soil, as it proceeds in depth, what other roots cannot attain, matter which, mixed with what the higher grounds bestow, serves to bring a variety to compound the different ingredients required for the various nourishment of the tree, probably minerals are wanted to form the juices of the *bark*; and I doubt not that the deep descent of the tap-root is most necessary to the *health* and *vigour* of the tree. How improper, then, is the custom of cutting it, and curtailing also many of the other roots, each of which has its appropriate branch, which will, of course, suffer in decay for the delapidations produced by the ignorance of the gardener. But the loss of the tap-root can never be remedied. It can no longer serve as a *deep well*, to gain not only a quantity of moisture from the number of rills it may meet with in its descent, but also matter from a variety of soil, and innumerable productions it passes in its way. The tap-root is, then, like the radicles, only a larger pump to collect and throw up all that it can select of water and other juices. The second part of the root is the reservoir for collecting the materials,

Third division, or tap root.

Use of the tap-root.

Never to cut the roots.

Defect of cutting the tap-root.

The radicles.

Root to be cut if injured.

Necessity of removing trees quickly.

rials; and the third part is the laboratory for forming each different gas and juice necessary to the health and habits of the tree. I may well add a fourth; for the radicles are the collectors sent out on every side to seek fresh provisions, to augment the stores, and increase the riches of this little habitation. They have all the mechanism appropriated to the purpose, and the bark and rind are so joined, as to serve, not only to protect and defend them from the stones and insects within the earth, but also to pierce and make way for them through the hardest materials; for they possess that softening power which enables them, I may say, to eat their way into every substance. It cannot be doubted, that they possess this faculty as well as *buds*, since I have perpetually found them dividing roots, piercing through the hardest wood, and even separating *stones* where any little defect assisted them. That a tap-root, or *any root* that is injured, should be *cut off*, there can be no doubt, since the danger of the rot is greater than any other inconvenience—but the greatest care (when trees are to be transplanted) should be taken not to hurt the roots, and if any radicles can be preserved, by wrapping them up in fresh earth, it should be done; for if they will live a little time, it *will be a great gain* to the tree; and here is the advantage of having the pit ready dug, and removing the plant, with all the earth around it—it preserves the few radicles alive, and enables them directly to perform their office of pumping moisture and nourishment from the earth—but if the tree is taken out some hours before it is replaced, all the *radicles* are sure to die; and if the tap-root is also injured, no wonder they never make fine trees; or that those planted by nature are always found superior. The reason that throwing a quantity of water into the pit has been found serviceable, *is*, that it supplies moisture, and quickens the growth of the new radicles; and what is still more advantageous, and should be *constantly done*, a large barrow of *good mould* should be thrown on the roots, and about the radicles; for a young and tender shoot, if it has to pierce through clods of earth in its sickly state, will certainly fail. It is like easily digested meat to a weak stomach—if you load it with heavy food at first, it destroys it at once; but let it gain strength and vigour, and a well-conditioned *radicle* will pierce through stone walls in time. It may be supposed, that according to the variety required to compound the juices of the tree, such is the depth or shallowness of the root. The oak and ash are two of the deepest rooted of our forest trees, and should not, therefore, be planted close together—they may injure by intersecting each others roots—indeed, the greatest care should be taken, that even trees of the same kind (much more if they are not so) should not shoot their branches so as to cross or lie on each other. It is inconceivable the mischief they do, whether root or stem branch—in the first, it is not so easy to guard against the evil; but at their original planting, great care should be taken to place their roots regularly and even in the ground, and not allow them to cross, in which case nature herself, with the utmost diligence, will avoid another root's covering them. But it sometimes happens both above and underground—and in the first, *when seen*, it should directly be remedied, for nothing brings the rot so soon. They either both contract into so small a compass as to injure each other, or one gets the better and destroys the other, or the dispute carries the rot into both—for they will not continue to lie one on the other, without receiving, or doing injury: but, first losing bark and rind, the upper one, pressing on the other, in a few years pierces it, and then the trial of strength begins between each separate set of the sap-vessels. I have some curious specimens of this kind, well worthy being presented to the public attention, as giving a thorough insight into the nature of a tree, and as admirably pointing out the consequential parts of a plant, which, of course, are always the last to give way.

Not to cross their roots,

and not to cross stem branches.

I shall now turn to the manner in which the roots of trees are folded (if I may so express myself) in most forest trees, such as the elm, the oak, the ash, &c. They are laid exactly like a circular fan, their folds meeting in the centre, and apparently doubled over at the bark. This is admirably seen in the oak, still better in the lime tree: but most visible in a good double microscope—and so excessive is their predilection for this shape, that cut them ever so straight, nay, *plane* them smooth, and in a few hours (if the wood retains any vigour of muscle) the ribs will again evidently appear to be rising, and the finger which passes over, will be able to mark its motion, or at least will feel the height the muscles have gained. In some roots I have measured the rising, and found it to be above the

Various folds of the root.

the tenth of an inch in twenty-four hours. Think what it must be in fresh and living plants! See the different sorts of figures into which the oak, &c. folds from that of the firs. The first fig. 2. BB, the second fig. 3. CC. showing it more plainly than in the circle. Let not the reader suppose, that this is the common warping of wood; it is the regular *fold*, always in one figure, and which goes off long before the wood is dry, and only retains it, like the animal muscles, a little while after life has ceased to linger in it. It is the last power of the muscular fibre of the wood, or rather of the spiral wire. I have not yet mentioned this as forming part of the root, because it does not (as in the stem) occupy a separate division in surrounding the nearest sap-vessels to the vital part. But in the wood of the root it circulates round *every sap-vessel*; separating each cylinder of wood, and meandering *on it* from one sap-vessel to another. (See fig. 4.) Besides penciling out the folds in three distinct rows of spiral wires, in each yearly increase, as at fig. 5. I doubt not, therefore, that it is the spiral wire which causes this peculiar motion, and I am the more persuaded of it, because, though the motion of the firs acts in such a reverse manner, yet the spiral wire accompanies it, so as equally to affect the motion, though in a perfectly *different direction*. What, then, can be said against the spiral wire being the cause of all motion in *plants?* The more I see of vegetable life, the more I am convinced of this reality. From the first I trusted to nature to *prove her own truths*; and she will do it, because I am most careful never to *make one for her*. The spiral wire never retains its power of motion above thirty-four hours after it is taken from a plant, and it is nearly the same with the root. I shall now indicate the extreme difference observed in the roots of firs, when compared with that part in other trees. I must beg Pliny's pardon for detecting him in an error when he says, "that he saw a fir tree planted, whose tap-root measured twelve feet." Now unfortunately, no fir has *any* tap-root. Such is the aversion their roots have to piercing the earth, that in young trees they will frequently be found with their roots bent back, and thus forming a hook or loop. (See fig. 6.) The roots of the Weymouth pine and silver fir, generally divide into *threes*, and run an amazing way horizontally under ground; but the Scotch and Spruce firs run

Folds caused by the muscular fibre.

Spiral wire which causes it.

No tap-root in firs.

in every direction, and a very little depth for such large and high trees. The division of the root, indeed, takes place in all the firs much higher in the stem than in other trees. None of the firs have any reservoir of alburnum, as is found in the second part of the root of the oak, &c. or I have not yet been able to discover it, from their not having a tap-root to guide me in the search; but I shall look more carefully. I have, however, dissected more than four dozen fir trees of all ages and sizes, growing naturally, and transplanted, not only in seeking *that*, but the tap-root also, but in neither have I succeeded. The tap-root in other trees, though, from being removed, it has branched and lost its shape, yet is always known; and so it would be in the firs, I doubt not, *if they had any*. But in all I have seen, there is not the smallest appearance of one, and if one of the side roots has been by accident turned down, its increased shape, on one side, shows that it was originally a side root forced into another situation—yet the *exact Evelin* also is mistaken. There is some variation in the bark and rind—they are not exactly like those of the stem. At first I thought they were not composed of leaves, as the coverings of the trunk of all firs are (see my letter on the subject in your Journal, 33;) but that was undoubtedly my mistake—they are formed of leaves, but thinner than the stem. The greatest part of the bark division is engrossed by that curious matter which separates the inner bark from the alburnum, and even in old roots is discovered to be of the most silvery whiteness; and its situation has so changed its very *nature*, that, instead of a thin division of hard rough wood, it appears like the most beautiful soft white leather. Surely its changing *thus* must show the excessive power of the juices of the firs in softening and tanning leather, and its vast superiority over the oak bark, or that of any other tree now made use of for the purpose—for though the oak has a very thin layer of this same matter to keep the debilitating juices from the wood, it is not softened and emolicated by its liquid, nor has it attained any thing like the suppleness or delicacy of that which encloses the fir, (as all may see that examine it) though they are both woods originally of nearly the same degree of roughness and hardness. I most sincerely wish that Sir H. Davy, or his brother, would turn their attention to this subject—it is very unlikely that nature would give

Dissected many firs.

Bark and rind not quite the same.

Superiority of the juices of the fir.

Wish that some chemist would try it.

give such a proof of this, if it was not thoroughly worthy the strictest attention. I have not yet described all the motion that belongs to wood in the roots: as this is equally found in firs as in all forest trees, I have retained it as the last explanation. It is inconceivable what alteration *two inches*, or *even one*, will sometimes produce in a piece of root. I have seen the pith change its place two or three times in as many inches. I have now a piece by me where all the ovals are within one another, and of sixty in number, change in less than three inches in length to three regular circles, each circle bearing interlacing figures of fifty-nine each. Conceive what must be the motion of wood, that would, in so short a space, produce such a revolution of *form* and *figure!* it was only in tracing each sap-vessel in length, that I could persuade myself of the reality; the stems of trees are infinitely more quiet in this respect, and generally retain the pith at the same side, unless some cause, such as rot, or any injury happening to force a change. But it is very different in the root; that perpetual motion appears necessary to it is certain, and I doubt not contributes to its health; and, by seeking further, we may be the means of developing many of the disorders of wood. How often we find, that laying the earth lighter on the roots gives fresh vigour to the tree: this knowledge of its motion will open a source of refreshment of great consequence, I should hope, to their general health. In short, the more we are acquainted with their inward structure, the more we shall be able to administer to their diseases. In this situation it may be compared to the advice given in a surgical case by a person a proficient in anatomy, and one wholly ignorant of it—he who is best acquainted with the formation is more likely to hit on the real disorder. May we not, therefore, hope, that, by gaining a thorough knowledge of the interior of plants, it may, in time, lead us to an acquaintance with their diseases, and give us some notion how to remedy them. I am collecting a set of specimens to show the different disorders in trees and their causes; and when I am advanced enough, shall lay them before the public.

Knowledge of the interior of plants

of use in gardening.

I am, Sir,
Your obliged Servant,
AGNES IBBETSON.

The roots of fruit trees, herbaceous and annual plants, will be given in my next letter.

References to the Engraving, Plate VIII.

Fig. 1. View of the five rows of alburnum found in the second part of the root.

Fig. 2. View of the sort of folding of the oak and all forest trees. Whether the shape of the wood be circular, oval, or any other shape, it always folds in this manner.

Fig. 3. View of the manner of folding of the firs: it all joins underneath, though so much doubled under.

Fig. 4. View of the piece of wood showing the spiral wire running round the sap-vessels as it does in every slender cylinder of the wood. They are so thin, that more than 150 may go to an inch in length.

Fig. 5. The wood showing the penciling of the folds by the spiral wire on the oak.

Fig. 6. The hook of a young root of fir.

Fig. 7. The root of the Weymouth pine reduced.

Great care should be taken, if the tree must come from the nurseries, not to plant them too old. It is astonishing, when you dissect wood, what a difference there is between wood thus planted, and wood never removed and growing from the seed; there is a regularity in the latter, an evenness of grain; particularly if it is all the same degree of *hardness*; not a piece almost iron in one place, and perfectly *soft* in another; it is all equally firm and solid. But I have repeatedly found in trees planted, large alternate layers of hard and soft wood, that must make it almost useless to the carpenter. There is also another defect, which arises from allowing people to cut off large branches from trees: the piece thus exposed will either decay and get the rot, or will grow as hard as stone in the middle, while all the circular part will be as soft as pith: if, therefore, three or four large branches are cut from a fine oak, at ever such a distance of time, it will render that tree extremely inferior, in point of wood, to that which never lost a large branch. If a branch grows too low, it should be cut off at first shooting, when it can have no bad effect. There is not in nature any thing which deceives so much, as *buying trees* while standing, unless the person is very knowing in wood, so various are the hidden and concealed infirmities they are filled with, that, till sawn out into boards, the juice is not to be known, and the wisest may be taken in.

III.

Popular Statement of the beautiful experiments of Malus, in which he has developed a new property of light.[*]

A solar ray is thrown in the meridian, to make an angle of 19° 10′ with the horizon: it is then reflected by a clear glass perpendicularly down:—and then by another clear glass, so as to pass with the same inclination to the horizon as before. Nothing remarkable happens if the ray be thus directed either N. or S. But if E. or W. the second glass reflects no light at all.

LET a solar ray be directed by means of a heliostat into the plane of the meridian, so that it shall form with the horizon an angle of 19° 10′. Then fix a glass, not silvered, in such a manner, that it shall reflect this ray vertically downwards. If below this glass a second glass be placed, exactly parallel to the former, the latter will make, with the descending ray an angle of 35° 25′, and reflect it again parallel to its first direction. In this case, nothing remarkable is observed.

But if this second glass be turned, with its face directed towards the east or west, but without altering its inclination with respect to the vertical ray, it will no longer reflect a single particle of light, neither from its first nor its second surface.

And if with the same inclination preserved with regard to the vertical ray, the face be turned towards the south, it will begin to reflect again the usual proportion of incidental light.

In the intermediate positions, the reflection will be more or less complete, accordingly as the reflected ray approaches more or less the plane of the meridian.

Under these circumstances, where the reflected ray comports itself so differently, it nevertheless constantly preserves the same inclination with regard to the incidental ray.

We, therefore, in this instance see, that a vertical ray of light falling on a transparent body, acts in the same manner when its reflecting face is turned towards the north or the south, and in a different manner when this face is turned towards the east or the west; although these faces still continue to form, with the vertical direction of this ray, an angle of 35° 25′.

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[*] See also Philos. Journal, XXX, 95. 161. 192.

These observations lead us to conclude, that light acquires, under these circumstances, properties independent of its direction, with regard to the surface which reflects it, but exclusively relative to the sides of the vertical ray, which are here the same for the north and south sides of the ray, but different with regard to the east and west sides.

Light thus acquires properties relative to the sides of the ray.

By giving to these sides the name of poles, Malus has given the name of *polarisation*, to that modification which imparts properties to light, which are relative to these poles. And he says, that he has hesitated to admit of that term in the description of the natural effects now under consideration, until the variety of the phenomena obliged him to make use of it.

Modification, which the author calls polarisation.

Let us again, says he, consider the apparatus of which we have been speaking. If to the solar ray which has passed through the first glass, and of which a part has been reflected, a silvered glass be presented, which shall reflect it perpendicularly downwards, a second vertical ray will be obtained, which has properties similar to the first, but in a directly opposite manner.

The light transmitted through the first glass is polarised differently.

If a glass be presented to this ray, forming with its direction an angle of 35° 25′, and if, without changing this inclination, its faces be alternately turned towards the north and south, east and west, the following phenomena will be observed. There will always be a certain quantity of light reflected by the second glass, but this quantity will be much less when the faces are turned towards the north and south, than when they are turned towards the east and west.

The reflection will be much less when the second glass faces north or south,

In the first vertical ray, exactly the contrary may be observed. The *minimum* of reflected light took place when the second glass was turned towards the east or the west. Thus, in abstracting from the second ray the quantity of light which comports itself like a common ray, and which is reflected equally under both circumstances, it will be seen, that this ray contains another portion of light, which is polarised in a manner exactly contrary to that of the vertical ray reflected by the first glass.

which is directly contrary to what happens with the first vertical ray.

In this experiment a silvered mirror was used, merely in order to dispose the two rays parallel to each other, and under the same circumstances, in order to render the explanation more clear. The action of metallic surfaces being very weak with

The silvered glass is not requisite, and has little effect on the polarisation.

regard to the polarisation of the direct ray, their influence may be neglected.

Statement of the effect in general terms.

This phenomenon in the last analysis may be explained in the following manner: If a ray of light fall on a plate of glass, and form with it an incidence of 35° 25′, all the light which it reflects is polarised in one direction or manner. And the light which passes through the glass is composed, first, of a quantity of light polarised in a direction or manner contrary to that which was reflected, and having a proportion to that quantity, and secondly, of another portion not modified, but which preserves the characters of direct light.

The rays receive the same modification as by the double refraction.

These polarised rays have precisely all the properties of those which are modified by the crystals which have double refraction; and, accordingly, what the author has said of them elsewhere may be applied without restriction to the former.

The author, by continuing his experiments on the polarisation of light, has observed the following facts:

Iceland spar used to distinguish the condition of the transmitted rays.

I consider, says he, in order to fix the ideas, a vertical ray as polarised with regard to the plane of the meridian, and I place beneath this ray a glass, not silvered, in such a manner, that it can be turned round upon the ray so as constantly to make with its direction an angle of 35° 25′. In order to analyse the light which is transmitted through this glass in its different positions, I place beneath it a rhomboid of Iceland spar, directing its principal section in the plane of the meridian. I shall call the *plane of incidence* that which passes through the vertical incident ray and the ray reflected by the glass.

Statement of the effect as shewn by the chrystal.

The ray presents different phenomena according to the motions or positions given to the glass itself. When the glass has made a quarter of a revolution, it no longer reflects a single particle of light, and the ray that it transmits to the lower crystal is refracted in the usual manner; and, subsequently, the reflected light diminishes, and the refracted light increases, from the first position of the glass, until the plane of incidence has described an arc of 90 degrees. The ray refracted in the ordinary way by the rhomboid, also increases from the former unto the latter position; but the extraordinary ray only increases until the plane of incidence has arrived at an angle of 45 degrees. It is then diminished, and becomes nothing when the glass has performed one quarter of a revolution. Supposing, then, that the glass makes one entire revolution, the reflected light will have two *maxima* answering to the positions N. and S. and two absolute *minima* answering to the positions E. and W. The transmitted light, and that which is refracted in the ordinary manner by the rhomboid, have two *minima* answering to the positions N. and S. and two *maxima* answering to the positions E. and W. but the light refracted extraordinarily has four absolute *minima* answering to the positions N. S. E. and W. and four *maxima* answering to the positions NW. SE. NE. and SW.

A metallic mirror substituted instead of the second glass.

In the place of the moveable glass, but under exactly the same circumstances, let a metallic mirror be substituted, of which the plane or incidence constantly makes an angle of 45° with that of the meridian. When this mirror is inclined only a few degrees with regard to the horizon, the light which it reflects is entirely polarised, like the incident light with regard to the plane of the meridian. If the inclination be augmented, it reflects, first, a certain quantity of light polarised with regard to the plane of the meridian; secondly, another quantity of light polarised with regard to the plane of incidence; and, lastly, a certain inclination may be attained, by which the light is completely polarised with regard to the plane of incidence. Beyond this limit, the light polarised with regard to the plane of the meridian begins to reappear, and the intensity of the light polarised with regard to the plain of incidence, diminishes until the mirror becomes vertical. Metallic bodies act, therefore, exactly in the same manner as transparent bodies on the light which they reflect; but transparent bodies totally transmit the light which they polarise in one direction or manner, and reflect that which is polarised in a contrary manner, while metallic bodies reflect the light which they have polarised in both directions or manners.

It polarises the light with very striking variations, governed by the angle of reflection.

The facts contained in this memoir point out the methods to be followed, in order to obtain, in the different cases, an exact measure of the phenomena. They resolve all that is problematical in this theory, and establish, in a decided manner, the following consequences—

That all bodies in nature, without exception, polarise completely the light which they reflect under a determined angle.

All bodies polarise the light, but the effect is governed by

That

angles of reflection peculiar to each. That within and beyond this angle the light receives this modification in a less complete manner.

It is essential to the reflecting force. Polished metallic bodies, which reflect more light than transparent bodies, do also polarise it in a greater degree. This modification is essential to the forces which produce reflection.

All the hypotheses to explain reflection are insufficient. Lastly, these new phenomena have advanced us one step nearer the truth, by confirming the insufficiency of all the hypotheses which philosophers have formed in order to explain the reflection of light. For example, it is certain, that not one of them tend to explain, why the most intense ray of light, when it is polarised, can, under a certain inclination, pass through a transparent body, and be totally deprived of the partial reflection to which ordinary light is subject.

IV.

Some Account of the Teak Tree of the East Indies. By Dr. WILLIAM ROXBURGH.

Introduction. THE durability of Teak wood for ship-building is well known to every one in India, and its qualities are so much valued in England, that considerable quantities are imported. The Society for Encouragement of Arts, from whose 30th volume the following paper is extracted, express their opinion, that this tree may be successfully cultivated in our West Indian and African settlements. And though it must be admitted, that the true national policy of an empire must ever be to establish those public resources which are least subject to the contingent events arising from the local distance of colonies, and consequently, that our great efforts ought to be to encourage the growth of native oak; yet it must be nevertheless admitted, that every possible means of insuring our supplies, and encouraging our settlements, ought to be adopted.

* * *

Great value of the teak tree of the East Indies. The timber of the teak tree is in India what oak is in England; it is, however, unnecessary to enlarge on their comparative value, because oak will not grow in India: our attention ought, therefore, to be confined to teak alone, not only as being by far the best wood we yet know of in this country for ship-building, but also for the house-carpenter, and almost every other work where strong, durable, easily-wrought, light wood is required. The advantages to be derived from the cultivation of so valuable a tree, where nature has not bestowed it, must therefore be obvious to every one; particularly in Bengal, where it grows well, and the demand is so great. The teak tree is a native of Pegu.

Encouraged by government.

Government, sensible of what is here stated, have long given every possible encouragement for an extensive propagation. But to render it still more general, the native land-holders must be made sensible of the advantages they may expect to derive from large plantations thereof.

Its growth is rapid.

The growth of the tree is rapid, and at all ages the wood (from various experiments) appears excellent. Some trees in the Honourable Company's Botanic Garden, brought from the Rajahmundry Circar in 1787, were, in 1804, from three to upwards of four feet in girth, at three and a half feet above ground, and high in proportion*. These plants were about twelve months old when sent from the coast, so that their present age is about seventeen years. A tree promising so much advantage in so short a space, compared to what the oak requires in England to become serviceable in the marine yard, makes it highly worthy of every attention and encouragement. A few observations on rearing the plants from the seed seem necessary, as I have often known seeds from the same tree succeed with one person, and totally fail with another.

Seeds, and manner of planting, &c.

The nut in which the seeds are lodged, is exceeding hard, contains four cells, and in each is lodged a single small seed. It has been ascertained, that they perfectly retain their vegetating power in the growth, even as far as eighteen months; however, it is advisable to sow them about the beginning of the first periodical rains, or north-westers, after they are taken ripe from the tree in October. If sown about this period, or rather before than after, in well-shaded beds, about an inch asunder, and covered with about a quarter of an inch of earth, with a little rotton straw or grass spread over the earth, to keep the beds in an uniform state of humidity, by gentle waterings, should the weather prove dry; most of the nuts will be found to produce from one to four plants, in from four to eight weeks. However, it sometimes happens, that many will remain in the ground until the commencement of the second rains, nay even of the third; however, this is rare, yet it will be adviseable to sow the seed on a spot that can be spared, at least until the rains of the second season are well advanced; by not attending to this circumstance, many have thought the seed bad, consequently caused the ground to be dug up for other purposes.

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* The largest of those trees measured, at three feet and a half above the ground, in February, 1796, forty-two inches in circumference. The same tree was, in February, 1804, fifty-two inches in circumference at the same place, which gives an annual increase of one inch and a quarter. However, while the trees are younger, and in a more favourable soil than where this tree stands, their yearly growth is from two to three inches, which is fully double the increase of oak in England.

Appearance of the plants.

The plants, when they first make their appearance, are very small, scarce so large as a cabbage plant when it first springs from the earth; their growth is, however, rapid. When they are about one or two inches high, they ought to be transplanted into other beds, at the distance of about six inches from each other, there to remain until the beginning of the next year's rains, when they are to be planted out to where they are to remain, or they may, when from two to four inches high, be planted out at once to where they are to grow; and it is not perfectly clear but by so doing they succeed better; as in taking up plants of any considerable size, say from one to two or more feet high, the roots are very apt to be injured, particularly the sap root, which retards their growth much, nay often kills them.

Time of transplanting.

Considerations relating to the proper soil.

About Calcutta they thrive luxuriantly in most places where they have been tried, and any tolerable degree of care taken of them; so that the only observations which seem necessary to be made on this head, are to avoid sowing the seed, or planting in such places as are low, or subject to be inundated; to keep them clear from weeds, and sparingly watered during dry weather, for the first year only. In a good soil, not much overrun with that coarse, white-flowered grass, called by the natives Woola (Saccharum,) they will scarce require any care whatever after the first six months, from the time of being planted out where they are to stand. They will then be about eighteen months old, supposing them to have been transplanted twice;

and in that time they will, in general, be from five to ten feet high, according as the soil is favourable, and out of all danger, except from north-westers.

Distances and thinning the plantations.

With respect to the distance at which plants ought to stand in plantations, every one's judgment can direct. The oak requires a great space, as the crooked parts thereof are the most valuable, and required for the knees and other curved timber in ship-building; but teak is naturally a straight-grained tree, and only used in Bengal, or at least in general, for the straight work, Sissoo being commonly employed for knees and other crooked timber; hence it may be concluded, that the straighter the teak trees grow, the more eligible for every purpose for which this timber is generally employed in Bengal. They do not, therefore, require to be planted at a great distance, suppose from six to ten feet, in quincunx order; by being so close they grow straighter, and protect one another while young, which is particularly wanted where violent gusts of wind, such as our north-westers, prevail. When the trees grow up, they can be thinned out to advantage, as the timber of the young trees will answer for a variety of uses. The seed of this tree we have now in such abundance, as to render a few hundred plants, in the hundred biggahs, of little or no importance; and if the ground on which they are planted is not of the best sort, the more necessity there is for planting close.

Suppose the trees planted in quincunx order, eight feet asunder, a Bengal biggah (which I believe is generally reckoned a square of one hundred and twenty feet) will hold about three hundred and ten trees.

Particular directions for the time of cutting down, &c.

It will be necessary, during the first ten years, to cut down about half of them, say one hundred and seventy, to give the rest more room; they are worth one rupee each.

Again, at from ten to twenty years, reckon half (eighty-five) of the remaining one hundred and seventy to be cut down, to make still more room for the rest, they will be worth four rupees each.

And again, at from twenty to twenty-five years, it may be necessary to thin them still more, say to another half, (or one-eighth of the original number) which will be worth eight rupees each. The remaining forty-two trees, when full grown, say in thirty years, may be expected to have, on an average, shafts or trunks thirty feet long, and at least four feet in circumference, which gives, according to the bases of timber merchants' measurement, a girth or square of twelve inches. The dimensions of such a piece of timber will therefore be thirty square feet, or three quarters of a ton, which, at one rupee (or about two shillings and sixpence sterling) per square foot, the average price of Pegu teak in this place (Calcutta) for some time past, will amount to thirty rupees per tree. Nor is it likely that the price of this indispensable commodity will fall; our growing trade, and consequent increase of shipping, gives reason to think it will rather rise in price. Let us, however, be on the safe side, and say, that each of the last-mentioned forty-two trees will be worth only twenty rupees each.

Value of the produce.

From the above statement, the value of a biggah of land, planted with teak trees, will produce, during thirty years, as follows:

	Rupees.
In the first ten years 170 are cut, and reckoned to be worth one rupee each, is - - - - -	170
In the next ten years 85 more are cut, and worth four rupees each, is - - - - - - -	340
In the next following five years 43 more are cut, and worth eight rupees each, is - - - - - -	344
At the end of thirty years, the remaining 42 trees are reckoned worth 20 rupees each, is - - - -	840
Total produce at the end of thirty years - - -	1694

Independent of the branches, many of the largest of which will be fit for knees, and other crooked timbers, of small dimensions, consequently of considerable value.

Reduction of rent, expences, &c.

From the above sum of 1694 rupees is to be deducted the rent of land for the before-stated time, together with the expence of planting, hedging, and taking care of the young plants during the first few years; after that they will require little or no care.

	Rupees.
The former let us suppose to be three rupees the biggah, which is certainly an high rent, and will amount, in thirty years, to - - - - - - -	90
Charges of planting and hedging, say - - -	20

Wages

Wages of one man, per biggah, which is fully sufficient, for the first five years, at 36 rupees yearly - -	180
For the next twenty-five years, allow one man to three biggahs, is for one biggah twelve rupees, or for twenfive years - - - - - - - -	300
Total charges of one biggah for thirty-five years -	590
Deducted from rupees 1694, leaves a clear profit of	1104

Potatoes, leguminous, and culinary plants, meliorating crops, may, with advantage to the plantation*, be reared in constant succession, on the same ground, during the first two or three years, or until the tops of the trees are too large to admit of their growth. The produce thereof will help to defray the expence of labouring the ground during that period; afterwards, as already observed, little more will be required than keeping up a fence round the plantation, to keep cattle and idle people from hurting the trees, till they are so large as to be out of all danger.

The ground may be profitably cultivated, and with advantage to the plantation.

A period of thirty years is only brought into the foregoing calculation, though it may well be imagined, that when in a healthy state, they must continue to gain considerably, both in size and quality, for a much longer period. In the *Bath Papers on Agriculture and Planting, Vol. 7, Article* 1, *Letter the Fourth*, a single oak tree is traced to have taken seventy-five years in acquiring a single ton; whereas in another seventy-five years, the same tree gave seven times as much in quantity, besides the increase in value as naval timber.

The observations relate to a period of thirty years. Subsequent years would give higher profit.

In addition to the remarks already made, it may be proper to add the following extract of a letter from Thomas Barnet, Esq. to G. H. Barlow, Esq. Chief Secretary to the Government, dated 8th November, 1799.

" A few years ago, a number of teak tree plants were, by

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* About six years ago my gardener trenched a piece of useless ground behind some cottages, and planted it with refuse elm suckers; thus prepared, the poor people availed themselves of the circumstance, set the ground with beans and potatoes, and have continued to crop it ever since; this has been of service to them, and of infinite benefit to the trees, which, by means of this annual culture, have outstript their undisturbed brethren, and almost doubled their contents. *Bath Paper*, *Vol.* 6, *p.* 17.

" orders of Government, I believe, disseminated in different " parts of the country, for the propagation of teak timber. " Amongst others, a few plants were sent to Rampore Bauleah; this was in 1795. These plants have throve in a surprising manner, and are, at this time, between twenty and " thirty feet high, and near a foot in diameter; the wood of " the hardest kind, and, as far as can be judged at present, " greatly superior to the teak of Pegu."

WILLIAM ROXBURGH.

Calcutta.
To C. Taylor, M. D. Sec.

V.

On some Combinations of Phosphorus and Sulphur, and on some other Subjects of Chemical Inquiry. By Sir Humphry Davy, *Knt. LL. D. Sec. R. S.*

1. *Introduction.*

The compounds of phosphorics and sulphur establish the notions of definite proportions in combinations, &c.

IN this paper I shall do myself the honour of laying before the Society the results of some experiments on phosphorus and sulphur, which establish the existence of some new compounds, and which offer decided evidences in favour of an idea that has been for some time prevalent amongst many enlightened chemists, and which I have defended in former papers published in the Philosophical Transactions; namely, that bodies unite in definite proportions, and that there is a relation between the quantities in which the same element unites with different elements.

I shall not enter into a minute detail of the methods of experimenting that I employed; I shall confine myself to general statements of the facts. The common manipulations of chemistry are now too well known to require any new illustrations: and to dwell upon familiar operations, would be to occupy unnecessarily and tediously the time of this learned body.

2. *Of some Combinations of Phosphorus.*

Two distinct

In a paper read before the Royal Society in 1810, I have de-

described the mutual action of phosphorus and oxymuriatic gas or chlorine. I have noticed two compounds which appear to be distinct and peculiar bodies, formed by the union of the gas and the inflammable substance. One is solid, white, and crystalline in its appearance; easily volatile, and capable of forming a fixed infusible substance by uniting with ammonia. The other is fluid, limpid as water, and, as I have since found, of specific gravity 1·45; it produces dense fumes by acting upon the water of the atmosphere, and when exposed to the atmosphere gradually disappears, leaving no residuum.

combinations of phosphorus and chlorine.

1. Solid, white, volatile: and when comb. with ammonia, infusible.

2. Limpid, sp. gr. 14·5, forming dense fumes with the water of the atmos. and evap. wholly.

The composition of the white sublimate is very easily ascertained by synthetical experiments, such as I have described on a former occasion in the Transactions. By employing chlorine dried by muriate of lime in great excess, and making the experiments in exhausted vessels, and admitting solution of chlorine to ascertain the quantity of gas absorbed, I have ascertained, that three grains of phosphorus unite with about twenty grains of chlorine to form the sublimate.

The first, or the sublimate, contains 3 of phosph. and 20 of chlorine.

If the phosphorus be in great excess in the experiment of its combustion in chlorine, some of the liquor is formed with the sublimate; but to obtain it in considerable quantities, phosphorus should be passed in vapour through heated powdered corrosive sublimate. A bent glass tube may be used for the process, and the liquor condensed in a cold vessel connected with the tube.

The second, or the liquor, is most plentifully formed by passing vapor of phosph. through hot corros. subl.

I have not been able to determine its composition by synthetical experiments; but by pouring it gradually into water, suffering the water to become cool after each addition of the liquor, and then precipitating the solution by solution of nitrate of silver, I have ascertained the quantity of chlorine and of phosphorus it contains. 13·6 grains, treated in this way, afforded 43 grains of horn silver.

It contains 3 phosph and 10 chlorine, or half the quantity contained in the sublimate.

It is evident, from this analysis, compared with the result of the synthetical experiments on the sublimate, that the quantity of phosphorus being the same, the sublimate contains double as much chlorine as the liquor.

When phosphorus is heated in the liquor, a portion is dissolved, and it then, when exposed to the atmosphere, leaves a film of phosphorus, which, when the liquor is thrown on paper, usually inflames: a substance of this kind was first procured

The liquor will dissolve phosphorus.

by MM. Gay Lussac and Thenard, by distilling phosphorus and calomel together; and it may be produced in the experiment with corrosive sublimate, if sufficient heat be used to sublime the phosphorus, or if there be not an excess of the corrosive sublimate. I have made no experiments, in order to ascertain the quantity of phosphorus the liquor will dissolve.

The sublim. combines with water, and after evap. affords sol. of phosphoric ac.

When the white sublimate is made to act upon water, it dissolves in it, producing much heat. The solution evaporated affords a thick liquid, which is a solution of pure phosphoric acid, or a hydrate of phosphoric acid.

The liquor, by similar treatment, affords crystals.

When the liquor is treated with water in the same way, it furnishes likewise a thick fluid of the consistence of syrup, which crystallizes slowly by cooling, and forms transparent parallelopipedons.

Gas emitted during its combustion,

This substance has very singular properties: when it is heated pretty strongly in the air, it takes fire and burns brilliantly, emitting, at the same time, globules of gas, that inflame at the surface of the liquid. This substance may be called *hydrophosphorous* acid; for it consists of pure phosphorous acid and water. This is proved by the action of ammoniacal gas upon it; when it is heated in contact with ammonia, water is expelled, and phosphate of ammonia formed; and it is likewise shewn by the results of its decomposition in close vessels, which are phosphoric acid and a peculiar compound of phosphorus and hydrogen.

which is hydrophosphorous acid.

Ten parts in weight of the crystalline acid I found produced about 8.5 parts of solid phosphoric acid, and the elastic product must of course have formed the remainder of the weight, allowing for a small quantity of the substance not decomposed.

Properties of the gas.

The peculiar gas is not spontaneously inflammable; but explodes when mixed with air, and heated to a temperature rather below 212°.

Its specific gravity appeared from an experiment in which a small quantity of it only was weighed, to be to that of air nearly as 87 to 100. Water absorbed about one-eighth of its volume of this gas. Its smell was disagreeable, but not nearly so fetid as that of common phosphuretted hydrogen.

When it was detonated with oxygen, it was found that three of it in volume absorbed more than five in volume of oxygen, and a little phosphorus was precipitated.

When potassium was heated in contact with it, its volume increased rapidly till it became double, and then no further effect was produced. The potassium was partly converted into a substance having all the characters of phosphuret of potassium; and the residual gas absorbed the same quantity of oxygen by detonation as pure hydrogen. When sulphur was sublimed in the gas over mercury, the volume was likewise doubled; a compound of phosphorus and sulphur was formed, and the elastic fluid produced had all the characters of sulphuretted hydrogen.

The gas consists of 4·5 oxyg. and 22·5 phosphorus.

It appears from these experiments, that the peculiar gas consists of 4·5 of hydrogen in weight to 22·5 phosphorus; and its composition being known, it is easy to determine the composition of the hydrophosphorous acid, and likewise the quantity of oxygen required by a given quantity of phosphorous acid to be converted into phosphoric acid; for, for every volume of gas disengaged, a volume of oxygen must have been fixed in the phosphoric acid.

Taking hydrogen as unity, the proportional no. of combustion of phos. will be 20.

And calculating for 174 grains, 30 parts of oxygen must be fixed in the 150 parts of phosphoric acid, and 20 parts of phosphorus disengaged in combination with 4 parts of hydrogen; and on the idea of representing the proportions in which bodies combine by numbers, if hydrogen be considered as unity, and water as composed of two proportions of hydrogen, 2, and one of oxygen 15*, phosphorus will be represented by 20.

Compound of chlor. and phos. acted on by water give mur. acid gas and oxyg. combines with the phos.

When the compounds of chlorine and phosphorus are acted on by a small quantity of water, muriatic acid gas is disengaged with violent ebullition, the water is decomposed, and it is evident that for every volume of hydrogen disengaged in combination with the chlorine, half a volume of oxygen must be combined with the phosphorus†; and the products of the mutual decomposition of water, and the phosphoric compounds of chlorine are merely the phosphoric acid from the sublimate, and the phosphorous acid from the liquor, and muriatic acid gas; so that the quantity of phosphorus being the same, it is evident that phosphoric acid must contain twice as much oxygen as phosphorous acid, which harmonizes with the results of the decomposition of hydrophosphorous acid. For supposing water to be composed of two proportions of hydrogen, and one of oxygen, and the number representing it 17; then 174 parts of hydrophosphorous acid must consist of two proportions; 34 parts of water, and four proportions of phosphorous acid, containing 80 of phosphorus and 60 of oxygene; and three proportions of phosphoric acid must be formed, containing three proportions of phosphorus 60, and six proportions of oxygen 90, making 150.

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* Supposing 100 cubical inches of the gas to weigh 27 grains.——27—4·5 the weight of 200 cubical inches of hydrogen = 22·5 grains.

† This mode of estimation is the same as that I have adopted on a former occasion, except that the number representing oxygen is doubled to avoid a fractional part.

And the facts shew that phosphoric acid contains twice as much oxygen as phosphorous acid.

Water and the phosphor. compounds shew completely the laws of definite combination.

It is scarcely possible to imagine more perfect demonstrations of the laws of definite combination, than those furnished in the mutual action of water and the phosphoric compounds. No products are formed except the new combinations; neither oxygen, hydrogen, chlorine, nor phosphorus is disengaged, and therefore the ratio in which any two of them combine being known, the ratios in which the rest combine, in these cases, may be determined by calculation.

Phosphoric acid produced by combust. in oxyg. contains 20 phos. and 30 6 oxyg.

I converted phosphorus into phosphoric acid, by burning it in a great excess of oxygen gas over mercury in a curved glass tube, and heated the product strongly. I found in several processes of this kind, that for every grain of phosphorus consumed, four cubical inches and a half of oxygen gas were absorbed; which gives phosphoric acid as composed of 20 of phosphorus to 30·6 of oxygen; a result as near as can be expected to the results of the experiments on the sublimate and the hydrophosphorous acid.

Unless the product of the combustion of phosphorus is strongly heated in oxygen, the quantity of oxygen absorbed is less, so that it is probable that phosphorous acid is formed, as well as phosphoric acid.

Common phosphorous acid is impure.

Phosphorous acid is usually described, in chemical authors, as a fluid body, and as formed by the slow combustion of phosphorus in the air; but the liquid so procured is, I find, a solution of a mixture of phosphorous and phosphoric acids. And the vapour arising from phosphorus in the air at common temperatures,

peratures, is a combination of phosphorous acid and the aqueous vapour in the air, and is not, I find, perceived in air artificially dried.

In this case, the phosphorus becomes covered with a white film, which appears to be pure phosphorous acid, and it soon ceases to shine.

A solid acid volatile, at a moderate degree of heat, may be produced by burning phosphorus in very rare air, and this seems to be phosphorous acid free from water; but some phosphoric acid, and some yellow oxide of phosphorus, are always formed at the same time.

Nature of the peculiar gas,

The peculiar gas differs exceedingly from phosphoretted hydrogen formed by the action of earths and alkalies and phosphorus upon water; for this last gas is spontaneously inflammable, and its specific gravity is seldom more than half as great, and it does not afford more than 1·5 its volume of hydrogen when decomposed by potassium; it differs in its qualities in different cases, and probably consists of different mixtures of hydrogen with a peculiar gas, consisting of 2 parts of hydrogen and 20 of phosphorus; or it must contain several proportions of hydrogen to one of phosphorus.

denominated hydrophosphoric.

I venture to propose the name *hydrophosphoric* gas for the new gas; and according to the principles of nomenclature, I have proposed in the last Bakerian lecture, the liquor containing 20 of phosphorus to 67 of chlorine may be called *phosphorane*, and the sublimate *phosphorana*.

3. *Of some Combinations of Sulphur.*

Sulphuretted hydrogen and sulphureous acid.

I have shewn, in a paper published in the Philosophical Transactions for 1810, that sulphuretted hydrogen is formed by the solution of sulphur in hydrogen, and I have supposed that sulphureous acid, in like manner, is constituted by a solution of sulphur in oxygen. There is always a little condensation of volume in experiments on the combustion of sulphur in oxygen; but this may fairly be attributed to some hydrogen loosely combined in the sulphur; and to the production of a little sulphuric acid by the mutual action of hydrogen, oxygen, and sulphur.

It is only necessary, if these data be allowed, to know the difference between the specific gravity of sulphureous acid gas and oxygen, and sulphuretted hydrogen and hydrogen, to determine their composition.

Weights of the gases,

In the Philosophical Transactions for 1810, page 254, I have somewhat under-rated the weights of sulphuretted hydrogen and sulphureous acid gases; for I have since found, that the cubical inch measures, employed for ascertaining the volumes of gas weighed, were not correct. From experiments which I think may be depended upon, as the weights of the gases were merely compared with those of equal volumes of common air, I found that 100 cubical inches of sulphureous acid gas weighed 68 grains at mean temperature and pressure, and 100 cubical inches of sulphuretted hydrogen 36·5 grains, and the last result agrees very nearly with one given by MM. GAY LUSSAC and THENARD, and one gained by my brother Mr. JOHN DAVY.

distinctly set forth,

If 34, the weight of 100 cubical inches of oxygen gas, be subtracted from 68, it will appear that sulphureous acid consists of equal weights of sulphur and oxygen, an estimation which agrees very nearly with one given by M. BERZELIUS; and if 22·7, the weight of 100 cubical inches of hydrogen be subtracted from 36·5, the remainder 34·23 will be the quantity of sulphur in the gas; and the number representing sulphur may be stated as 30; and sulphureous acid as composed of one proportion of sulphur 30, and two of oxygen 30; and sulphuretted hydrogen as composed of one proportion of sulphur, and two of hydrogen.

and their numerical composition.

Other experiments considered.

From the experiments of MM. GAY LUSSAC, it appears that sulphuric acid decomposed by heat affords one volume of oxygene to two of sulphureous acid: from this it would appear to be composed of one proportion of sulphur to three of oxygen. I have endeavoured, in several trials by common heat and by electricity, to combine sulphureous acid gas with oxygen, so as to form a sulphuric acid free from water, but without success; and it is probable, that three portions of oxygen cannot be combined with one proportion of sulphur, except by the intermedium of water. Mr. DALTON has supposed, that there is a solid sulphuric acid formed by the action of sulphureous acid gas upon nitrous acid gas. But I find, that when dried sulphureous acid gas and nitrous acid gas are mixed together, there is no action; but by introducing the vapour of water, they form together a solid crystalline hydrate; which when thrown into water gives off nitrous, and forms a solution of sulphuric acid.

Chlorine and sulphur.

I have referred, in the Philosophical Transactions, to the combination of chlorine and sulphur. I have been able to form no compound of these bodies, which does not deposit sulphur by the action of water. When sulphur is satured with chlorine, as in Dr. THOMSON's sulphuretted liquor, it appears to contain, from my experiments, only 67 of chlorine to 30 of sulphur.

4. *Some general Observations.*

Observations. The proportions in phosphoric and sulphuric acid are the same, though the attractions greatly differ.

It is a fact worthy of notice, that phosphoric and sulphuric acids should contain the same quantity of oxygen to the same quantity of inflammable matter; and yet that the oxygen should be combined in them, with such different degrees of affinity. Phosphorous acid has a great tendency to unite with oxygen, and absorbs it even from water: and sulphureous acid can only retain it when water is present.

Most precipitates from water contain that ingredient.

The relation of water to the composition of many bodies has already occupied the attention of some distinguished chemists, and is well worthy of being further studied; most of the substances obtained by precipitation from aqueous solutions are, I find, compounds of water.

Thus zircona, magnesia, silica, when precipitated and dried at 212° still contain definite proportions of water. And many of the substances which have been considered as metallic oxides, that I have examined, obtained from solutions, agree in this respect; and their colours and other properties are materially influenced by this combined water.

Instance.

I shall give an instance. The substance which has been called the white oxide of manganese is a compound of water and the protoxide of manganese, and when heated strongly, it gives off its water and becomes a dark olive oxide.

Contraction of the pure earths by heat arises from the expulsion of water.

It has been often suspected, that the contraction of volume produced in the pure earths by heat, is owing to the expulsion of water combined with them. The following fact seems to confirm this suspicion, and offers a curious phenomenon.

Zircona, precipitated from its solution in muriatic acid by an alkali, and dried at a temperature below 300°, appears as a white powder, so soft as not to scratch glass. When heated to 700° or 800°, water is suddenly expelled from it, and, notwithstanding the quantity of vapour formed, it becomes at the moment red hot. After the process, it is found harsh to the feel, has gained a tint of gray, its parts cohere together, and is become so hard as to scratch quartz.

SCIENTIFIC NEWS.

Geological Society.

Geological Society.

AT the meeting of this Society, Dec. 4th (the president in the chair) the reading of a paper by William Phillips, Esq. M. G. S. "on the views of Cornwall," was begun.

The regular or metalliferous views of Cornwall are found, with few exceptions, to run east and west. The known length of many of these views is considerable, amounting, in some instances, to two or more miles; but their actual termination at either extremity has in no case been satisfactorily ascertained; all that is known being, that they gradually become so poor and narrow, as to make it no longer worth the miner's while to pursue them.

The dip or descent of the veins varies more or less from perpendicular, inclining towards the north or south, which inclination is called the *underlie* of the lode.

The depth of the veins is still less known than their longitudinal extent, not an instance having occurred of a vein being fairly worked out: many veins have indeed been relinquished, but only on account of the expences of working them exceeding the produce. The deepest mine now in work in Cornwall, is Dolcoath, some of the workings of which are 228 fathoms below the surface.

The usual width of the veins that are worked, varies from one foot to three; in particular instances, however, portions of veins occur twenty-four feet, and even thirty feet wide; and, on the other hand, a vein of tin, not three inches wide, has been followed with profit.

The

Geological Society.

The substances that accompany the metallic ores (or the vein-stones) vary considerably, not only in different mines, but in different parts of the same vein; and it is from these, and not from their metallic contents, that the miner's nomenclature of the veins is derived.

Gossan is a friable substance of a loose texture, consisting of clay, mixed more or less with silicious matter, and coated or tinged with oxide of iron. Its colour varies from light yellow to deep and brownish black. A *gossany* lode is more common than any other, and is considered as promising both for copper and tin.

When quartz predominates, the vein is called *sparry*; and if the quartz is considerably compact, it is looked upon as a very unfavourable indication, more especially if the vein becomes narrower as it descends.

If iron pyrites abounds, the vein is said to be *mundicky*. When this substance occurs at a shallow level, it is considered as not unpromising, more especially if mingled with copper ore as it descends.

A vein containing a large proportion of chlorine, is termed a *peachy* lode, and promises for tin rather than copper.

A vein is said to be *flookany* when one or both of its sides is lined with bluish white clay. It sometimes is so abundant, as to occasion considerable difficulty and expence to prevent it from slipping down, and obstructing the works.

When the contents of a vein consist of a hard compact substance, of a greenish or brownish colour, which appears to be chiefly a mixture of quartz and chlorite, the vein is denominated *caply*. Tin is often found in it, copper rarely.

When the ore, whether of tin or copper, is found in detached stones or bumps, mixed loosely with the other contents of the vein, it is termed a *pryany* lode.

A vein abounding in blende, is called a *Black Jack* lode, and is considered as unpromising for tin, but a good sign for copper.

When a vein contains granite in masses or blocks, or in a state of semi-decomposition, it is termed a *growan* lode; and is generally considered as more promising for tin than for copper. Of late, however, many rich veins of copper have been found in the granitic districts of Cornwall.

The experienced miner by no means implicitly relies on even

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the most promising symptoms, for all of them at times are found to mislead. The following, however, are those, in favour of which he is more especially prepossessed. All gossany lodes in general; the early discovery of pyrites with portions of yellow copper ore, also of blende and of gelina; and the cutting a good course of water, especially if it be warm.

The discovery of veins is effected in various ways.

The ancient method of *shoding* or tracing up water-courses, when pieces of ore are found to occur among the rolled stones in their channels, is now rarely resorted to. The common method is to work drifts across the country from north to south, by which all veins in the district thus examined, are sure to be cut through. Veins are often found in driving adits and levels for the working of known lodes; and not unfrequently are stumbled upon by mere accident in digging ditches and foundations for walls.

December 18.

The president in the chair.

The continuation of Mr. Phillips's paper on the Veins of Cornwall, was read.

The contents of a vein may be divided into those which are valuable, and those which are not so: the latter forming generally by far the largest portion, are technically called *deads*, and are left in the vein both to avoid the unnecessary expence of raising them to the surface, and for the very important purpose of preventing the two walls of the vein from collaping, and thus destroying the works: in addition to the deads, strong pieces of timber are frequently made use of. Sometimes large wedged-shaped fragments of rock, called by the miner *horses*, occur in the vein, partially cutting off the regular contents of lode, though seldom, if ever, entirely obstructing it. Veins of copper ore are, however, particularly liable to capricious and total obstructions, without any obvious cause. In proportion as the rock becomes harder, the vein always becomes more narrow.

One of the first objects in opening a new mine, is to drive an *adit* or horizontal gulley from the lowest convenient level, for the purpose of carrying off all the top water. One adit often serves two or three mines; and there is one, called the deep adit, which opens on one of the creeks of Falmouth harbour, the entire subterranean length of which is about twenty-four miles. Geological society.

Copper veins, which, fifty years ago, were considered by the Cornish miners to be peculiar to Schist, have, of late, been found in the parishes of Givenass and Redruth to pass freely from Schist into Granite, and back again to Schist without any deterioration. The texture and hardness of both rocks is liable to considerable variation, affecting, of course, the profit and progress of the miner often in a very remarkable degree. Two shafts of Fluel Alfred were sunk in Schist, and the cost of one did not exceed 5l. per fathom, while that of the other amounted to 55l. for the same length.

The metalliferous, or east and west veins, are crossed by others, the direction of which is nearly north and south. These latter are called *cross courses,* and rarely produce copper or tin, or any other metallic substance. The principal practical advantage derived from these veins, especially when consisting of clay, is, that they oppose an effectual obstacle to the passage of water, and therefore the miners do not willingly pierce them without some adequate object in view. The disadvantage of them is, that they not only interrupt the course of the metalliferous veins, having them from a few inches to several fathoms; but not unfrequently totally impoverish them, so that a long and costly search after the heaved part of a vein, often terminates in the mortifying discovery, that it is not worth pursuing, as was most strikingly exemplified in the corresponding veins of Huel Jewel and Tol Carn.

There is another species of vein called a Contre or Caunter, the direction of which is, for the most part, NE. and SW. These are mostly, if not always, metalliferous, and often remarkably rich, of which the mines of Huel Alfred and Herland have afforded most splendid instances.

Phosphorescence of Bodies.

Deseignes continues his experiments on the phosphorescence of bodies. It was formerly published, that he found, that by violently compressing water in a glass tube, by a blow, it became

came luminous. He has made the same experiment with a great number of liquids, which became luminous by the same treatment; such as olive oil, volatile oils, alcohol, sulphuric ether, acetous, and a saturated and boiled solution of potash, &c.

He ascertained, at the same time, that the temperature of all these liquids was at the same time raised.

Solid bodies likewise become luminous by compression. He filled the same tube with powdered chalk, and gave it a blow in the dark. The whole mass was then penetrated with a strong light, which disappeared like a flash of lightning.

He had the same results from flowers of sulphur, dried sulphate of magnesia, nitrate of potash, black oxide of manganese, ashes, powder of mica, and of vegetable coal, &c. and, in a word, every thing that was at hand.

The same bodies, struck with an hammer or an anvil, likewise gave atmospheric light; but particularly fluat of lime, phosphate of lime, and caustic lime; but sulphur, the metallic oxides by calcination, and burned alum, gave a very feeble light.

This difference seemed to him to have arisen from water in the solid state contained in these bodies. He ascertained this by the following experiments. He poured a few drops of water upon caustic lime, and it became very luminous by the blow. And the same effect was produced on other bodies.

Other experiments showed, that this light produced by compression is not electrical, but arises from the sudden approach of the particles of the bodies to each other.

Questions from a Correspondent on Subjects tending to encourage the Iron manufacture of this Country.

Has the making of iron had a gradual increase since 1800? and since that year, have considerable works been erected or established in addition throughout the United Kingdom, and principally where?

What quantity of iron may be now made in England, Wales, Scotland, Ireland—this is, iron into *bars* ?

What may be the quantity in same manner of *cast iron* into various purposes for domestic and general use ?

No doubt the British ore, if worked by wood, could produce equal iron to any we import ; but is the quality of iron improved by working with coal at the present period, and is it possible by coal to make it equal to the foreign we import ?

Is any quantity of British cast ware exported, as well as British iron in bars, and where to principally ?

Is it not possible to manufacture all the iron we require for home consumption and exportation amongst ourselves ? and by what means could it be adopted ?

Does not the duty on the importation of foreign iron act as a bounty upon our own ? or does the importation of foreign iron interfere with that of our own manufacture ? and what means would be the most effectual to depress the importation of foreign, by a competition in the manufacture of our own ?

Is it possible to increase the making of iron in the United Kingdom adequate to all wants and exportation without the danger of exhausting our own native resources, if the iron trade could be made the principal staple of the country ?

In what part of the United Kingdom can the Iron be made the cheapest ?

What may be the whole aggregate quantity of Iron, *cast in bars* in various iron utensils ; and the whole manufacture of iron ware, from *native ore*, throughout the United Kingdom ?

If the greatest quantity of iron is made in Wales, what may be the expence of conveying it up to London ?

Meteorological Table for 1812. *Extracted from the Register kept at Kinfauns Castle, the residence of Lord Gray, three miles almost due East from Perth, N. Britain, about ninety feet above the level of the ray.—Lat.* 56' 24'.

Communicated by his Lordship.

1812.	Morning, 8 o'clock. *Mean hight of.* Barom.	Morning, 8 o'clock. *Mean hight of.* Ther.	Evening, 10 o'clock. *Mean hight of* Barom.	Evening, 10 o'clock. *Mean hight of* Ther.	Depth of Rain In : 100	No. of days. Rain or snow.	No. of days. Fair.
January.	29·92	30·60	29·94	30·70	·72	7	24
February.	29·64	37·55	29·67	37·31	2·16	17	12
March.	29·96	33·46	29·99	33·30	·86	17	14
April.	30·09	38·40	30·11	36·71	1.38	8	22
May.	30·02	48·20	30·02	45·45	1·41	17	14
June.	30·01	54·17	30·02	52·00	2·89	12	18
July.	30·04	55·22	30·05	52·97	2·56	15	16
August.	30·09	55·10	30·10	53·16	2·38	11	20
September.	30·03	52·00	30·03	49·00	1·06	6	24
October.	29·47	45·00	29·50	45·10	3·18	16	15
November.	29·89	37·76	29·91	38·10	3·50	14	16
December.	30·09	34·00	30·14	35·00	·65	9	22
Average of the year	29·937	43·43	29·953	42·40	22·75	149	217

In the course of the present month, will be published, in one volume octavo, a Treatise on the Motion of Rockets, together with the Theory and Practice of Naval Gunnery; by W. Moore, of the Royal Military Academy, Woolwich.

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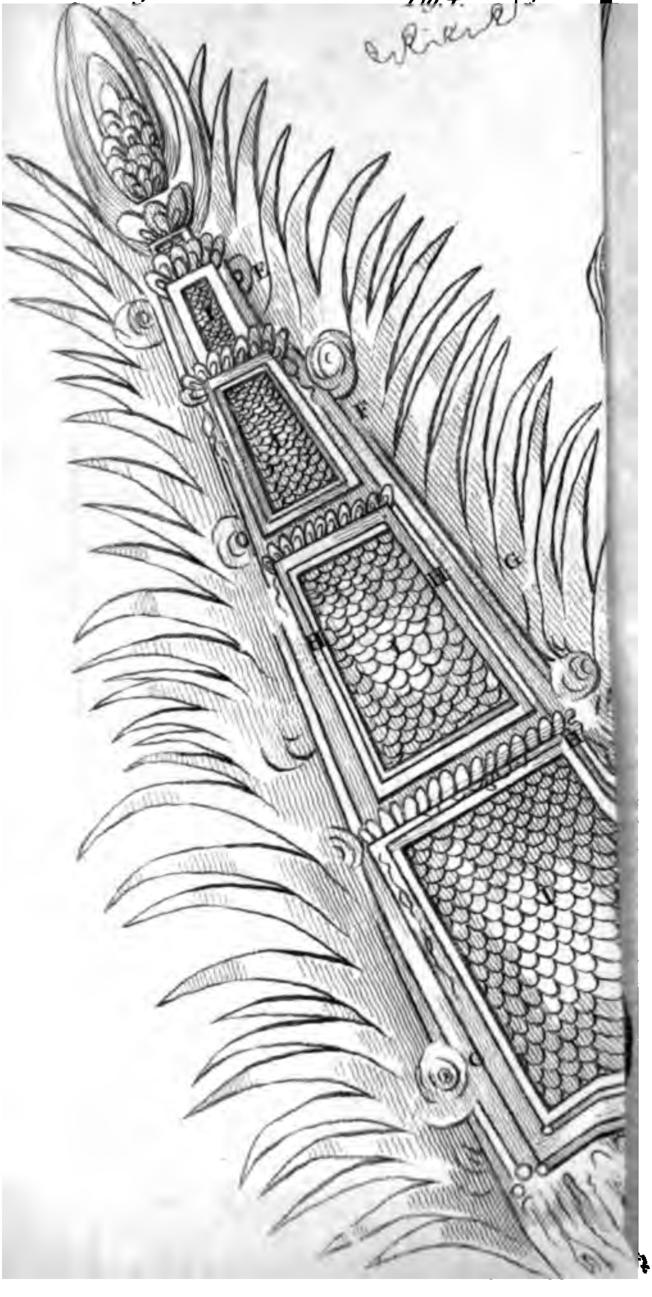

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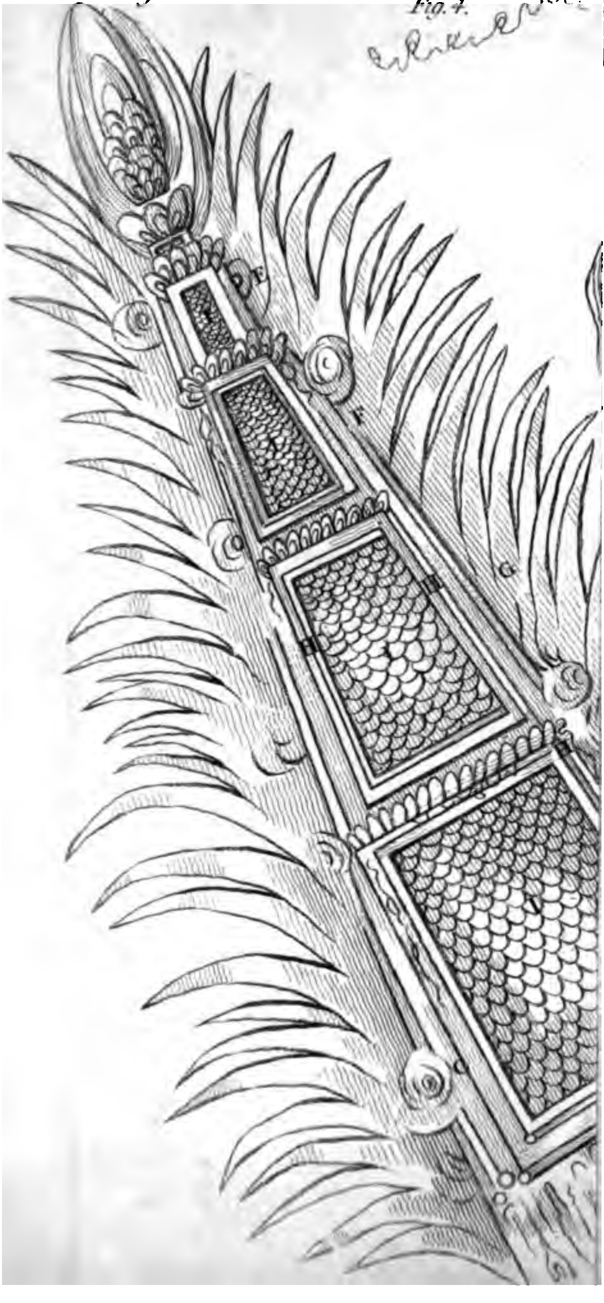

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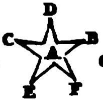

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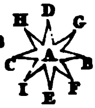

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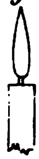

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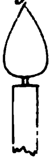

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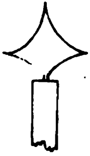

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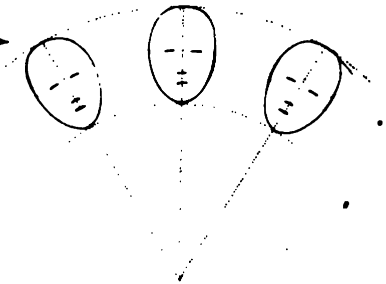

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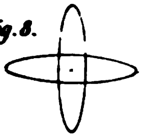

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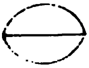

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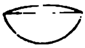

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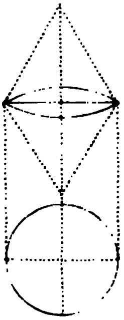

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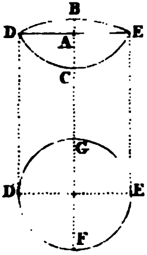

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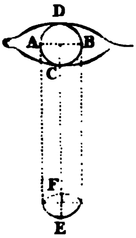

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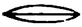

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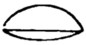

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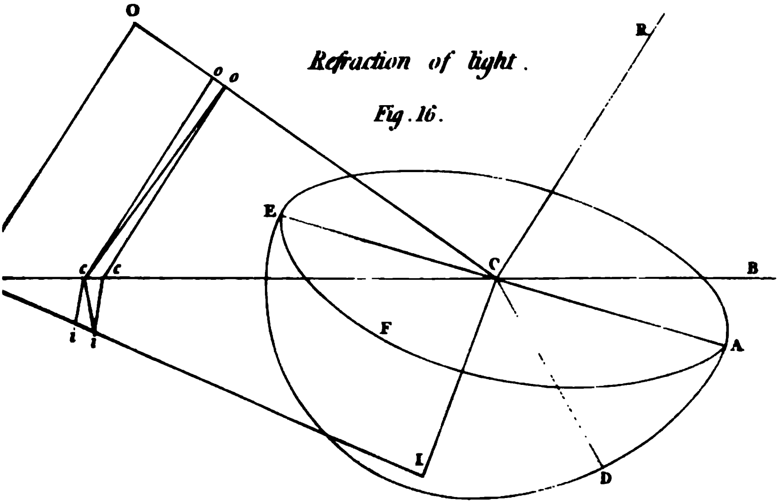

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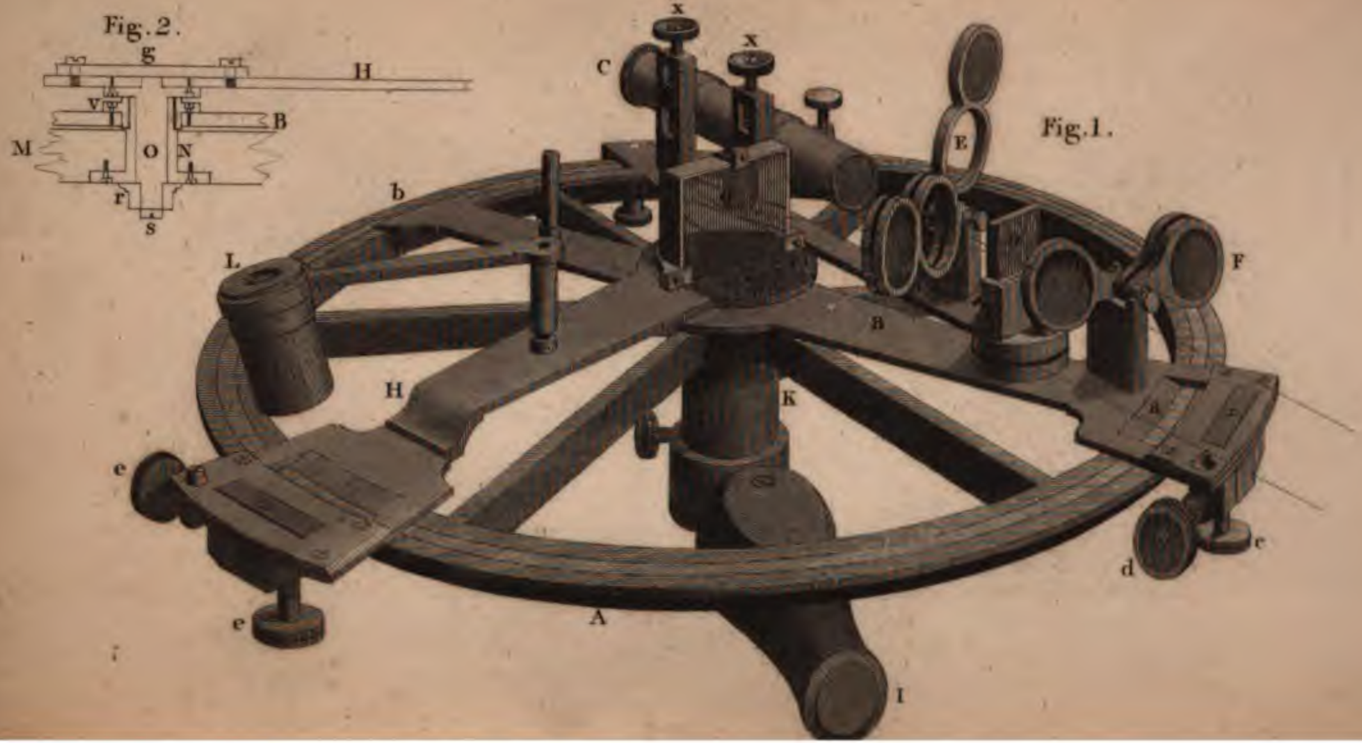
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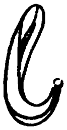

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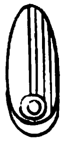

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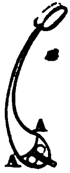

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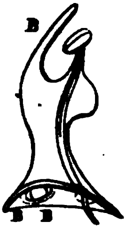

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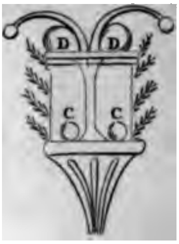

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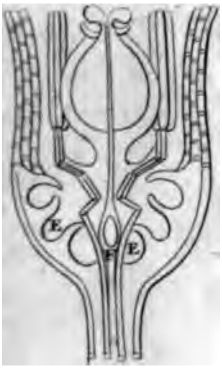

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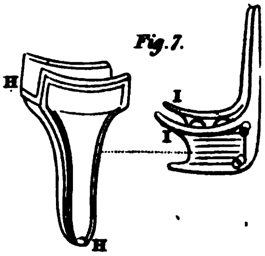

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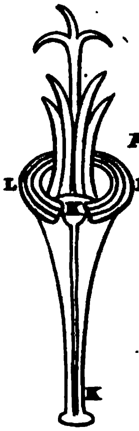

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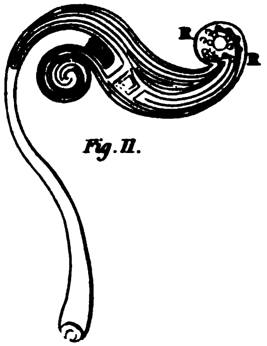

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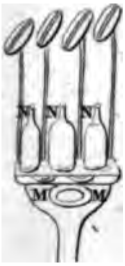

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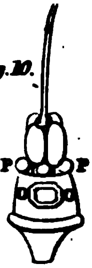

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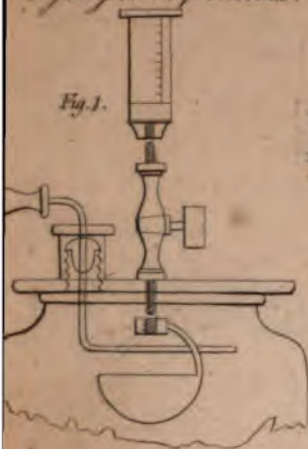

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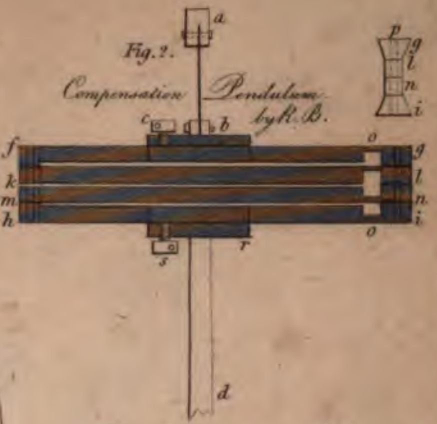

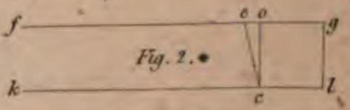

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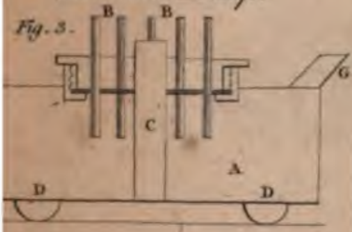

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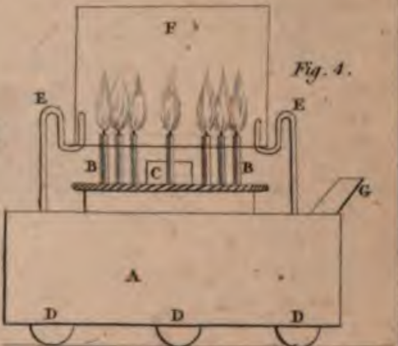

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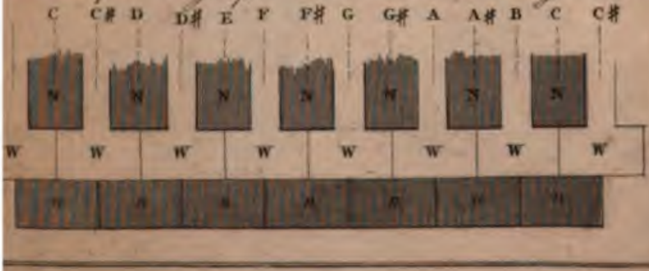

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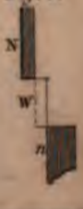

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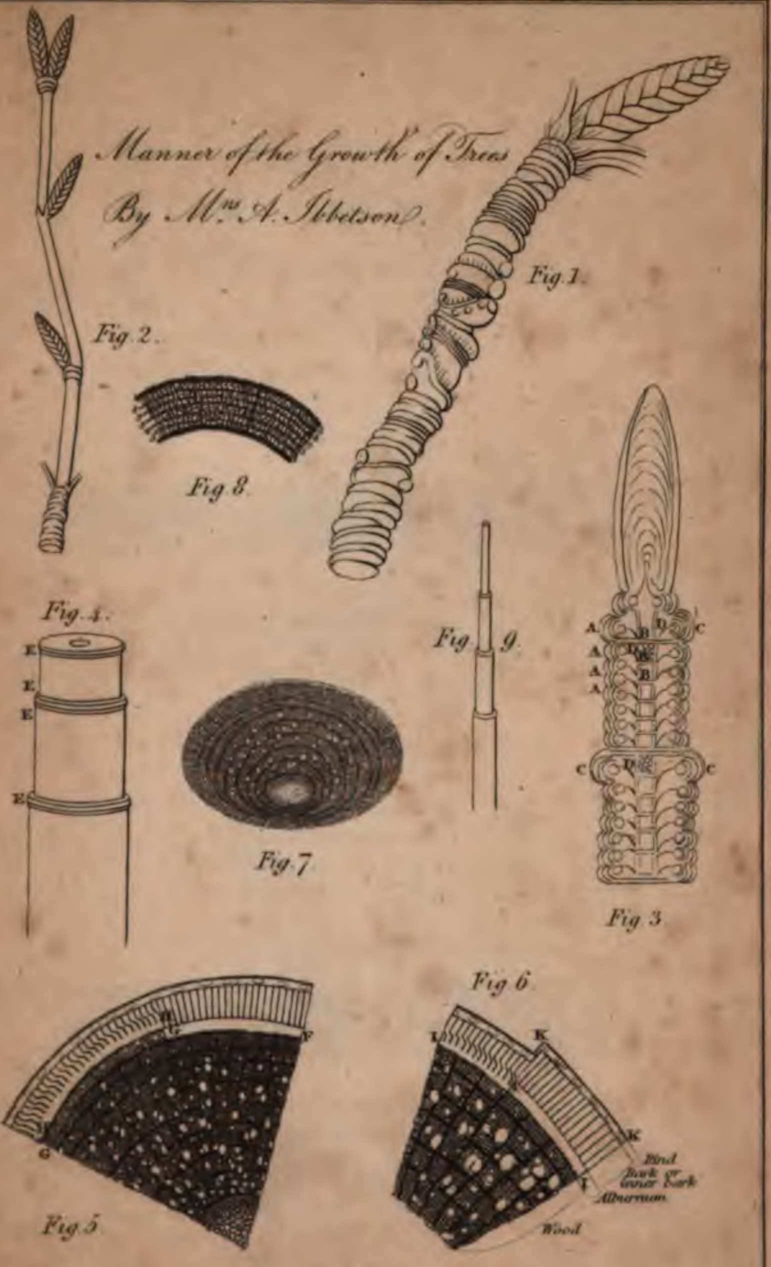

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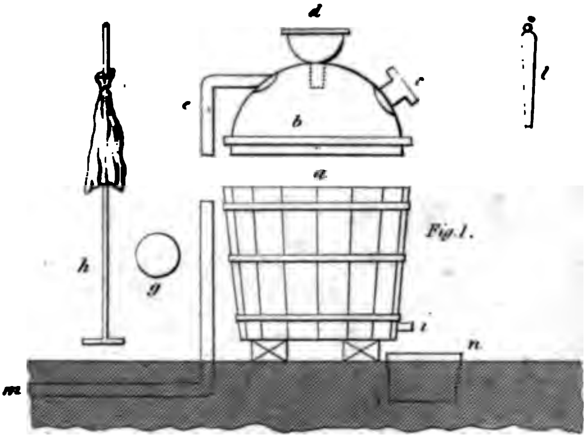
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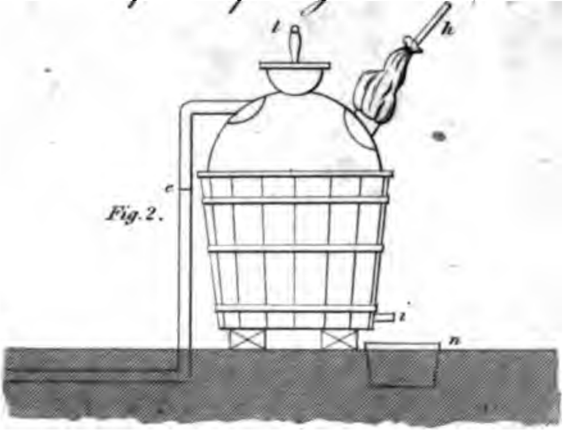
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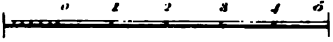

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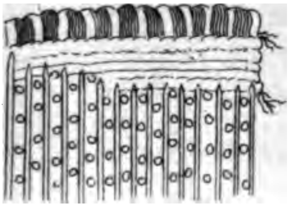

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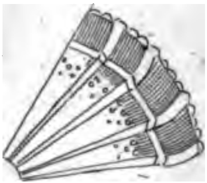

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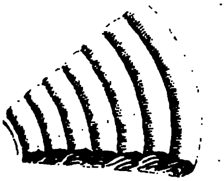

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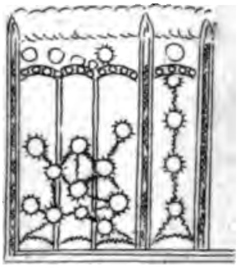

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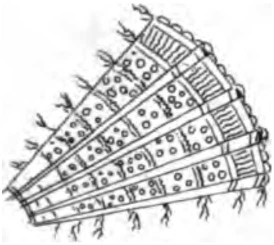

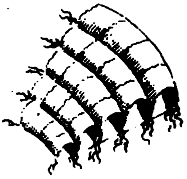

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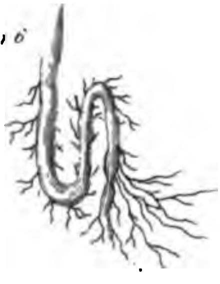

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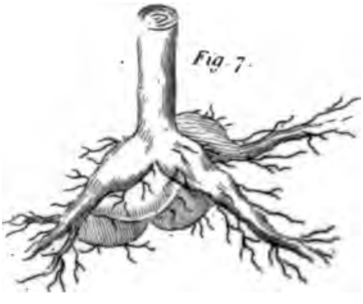

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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JANUARY, 1813.

ARTICLE I.

Comparative Analyses of the Urine of different Animals. By Mr. VAUQUELIN[*].

Few kinds of urine satisfactorily analysed.

THE only kinds of urine, that chemists have hitherto analysed in a satisfactory manner, are those of man, and some of the larger herbivorous animals. Those of the carnivorous animals and *glires* have not yet been examined by any person that I know of.

Comparative chemistry recommended.

If it be acknowledged, however, that comparative anatomy has contributed much to the advancement of physiology, it will also be found, perhaps, that comparative chemistry may be of great advantage to that science.

Urine of birds.

Already has the analysis of the urine of birds afforded results sufficiently interesting and unexpected, to induce chemists to pursue the inquiry in all animals that furnish this fluid, that we may not judge from analogy, which is frequently deceitful. It is with this view, that I have undertaken the analysis of the urine of the royal tiger, the lion, and the beaver; the results of which I here give, till I have time to pursue my inquiry on this subject farther.

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[*] Ann. de Chim. vol. LXXXII, p. 197.

Urine of the lion and the royal tiger.

Urine of the lion and tiger.

The urine of the lion, and that of the tiger, are perfectly similar in every respect. They have likewise some analogy to that of man, but they differ from it essentially in some important points.

Points in which they differ from that of man.

First difference.—They are alkaline at the very instant they are voided: on the contrary, those of a healthy man are constantly acid.

Ammonia.

It is to the presence of ammonia developed in these urines, that we must ascribe the strong and disagreeable smell they diffuse immediately on issuing from the bladder of these animals.

No uric acid:

Second difference.—They contain no uric acid, either free or combined with an alkali. At least the analysis of these urines four times repeated afforded me no sensible trace of it.

animal food therefore not its source.

The want of uric acid could not but the more attract my attention, as I had considered its formation to be owing chiefly to animal food.

Want of phosphate of lime.

Third difference.—The almost total absence of phosphate of lime.

This might naturally be expected, as this salt is soluble in water only by the help of a superabundance of acid, and the urine in question is alkaline.

Yet this is separated in the kidneys; but probably precipitated by the ammonia.

It appears, however, that the kidneys of these animals separate a certain quantity of this salt from the blood, for I have found slight traces of it in these urines; while the ammonia is formed only in the bladder, where probably it precipitates the phosphate of lime; and this is no doubt the reason why the urine of these animals is almost always turbid when voided.

Their calculi must be phosphate of lime.

Hence, if calculi be ever found in the bladder of these animals, they can be formed only of phosphate of lime, since they contain no other insoluble substance.

Little muriate of soda.

Fourth difference.—The urine of the lion and of the tiger contains but an infinitely small quantity of muriate of soda, while that of man commonly affords much.

Much urea. Other substances.

In these urines we find a large quantity of urea, much disposed to crystallize, and in general but lightly coloured; phosphates of soda and ammonia; sulphate of potash; a mucous matter, and a trace of iron.

These are the points in which the urine of the lion and the royal tiger resembles those of man: but it differs from it, as we have seen, in a sufficient number of points, to consider it as a distinct species.

It is composed of

Component parts.

1, Urea,
2, Animal mucus,
3, Phosphate of soda,
4, ———— ammonia,
5, Muriate of ammonia,
6, A trace of phosphate of lime,
7, Sulphate of potash in large quantity,
8, An atom of muriate of soda.

Urine of the beaver.

Urine of the beaver.

A careful analysis of the urine of the beaver, several times repeated, has taught me, that it has a great similitude with the urine of the common herbivorous animals.

Its contents.

In fact, we find in it carbonate of lime held in solution by a superabundance of carbonic acid; the benzoic and acetic acids; urea, muriate of soda, and sulphate of potash: but no uric acid, or phosphoric salt.

Difference from that of common herbivorous animals.

It differs from them, however, in containing no muriate of ammonia, but a notable quantity of carbonate and acetate of magnesia, which are not found, at least in any great quantity, in the urine of herbivorous animals.

The following is the mode in which I detected the carbonate of magnesia.

Mode in which the carbonate of magnesia was detected.

After having concentrated a certain quantity of the urine by a gentle heat, I decanted the thickened liquor, and washed with distilled water the vessel, on the sides of which the carbonate of lime had settled. I then poured in some sulphuric acid, diluted with water, which produced a frothy effervescence, on account of a mucous matter, which the carbonate of lime carries with it.

Perceiving that the sulphuric acid had acquired a bitter taste in this combination, I dried and calcined the mixture, washed it with a little water, and by evaporation obtained a salt, that possessed all the properties of sulphate of magnesia.

Desirous

Desirous of knowing by another experiment, whether the urine of the beaver, like that of all other herbivorous animals, contained any muriate of ammonia, I put into a portion of the thickened liquor a bit of caustic potash; and as no smell of ammonia was perceived, even when heat was applied, I concluded, that it contained no muriate of ammonia. But a phenomenon presented itself, that occasioned me some surprise, and made me desirous of discovering its cause. The liquor coagulated into a gelatinous mass. Suspecting that this effect was produced by the precipitation of some earthy substance, I treated the whole of the thickened urine I had with caustic potash; filtered off the liquor to obtain the matter in question; and after it was washed and calcined, combined it with sulphuric acid, diluted with water, and obtained sulphate of magnesia mixed with a little sulphate of lime.

The acetate of magnesia perhaps a product.

Though I have said, that the urine of the beaver contains acetate of magnesia, yet I am not perfectly certain of it. It is possible, that during the evaporation, though effected by a gentle heat, a certain quantity of acetic acid was formed; and that this acted on the carbonate of magnesia, remaining in the liquor in consequence of its being more soluble than the carbonate of lime.

Colouring matter of its food found in the urine.

We commonly find by the colour, smell, and taste of the beaver's urine, and particularly by its property of dying alumed stuffs, the kind of vegetable on which it has fed.

Instance.

In that in question I very evidently distinguished the colouring matter of willow bark, and its keeper afterward confirmed my observation.

Properties of vegetables not always destroyed in the circulation.

There are cases, therefore, in which certain vegetable substances are capable of passing the digestive organs and the circulation, without losing the properties that distinguish them in their natural state.

Presence of iron.

I found also in the urine of the beaver a quantity of iron, that at first astonished me: but on reflecting, that it had been saved in a tin vessel, and that it contained carbonic acid, I believe the greater part of the metal must be ascribed to this vessel.

The urine of the beaver, then, is composed of

Component parts of the urine.

1, Urea,
2, Animal mucus,
3, Ben-

3, Benzoate of potash,
4, Carbonate of lime and of magnesia,
5, Acetate of magnesia (questionable),
6, Sulphate of potash,
7, Muriate of potash and of soda,
8, Colouring vegetable matter,
9, A little iron.

II.

Some Account of ZERAH COLBURN, *an American Child, who possesses some very remarkable Powers of solving Questions in Arithmetic by Computation, without Writing, or any visible Contrivance.*

[The present article is copied from a printed paper, which I obtained from Messrs. Johnson and Co., booksellers, in St. Paul's Church-yard. This boy has been publicly exhibited in America and in London, and some time ago subscriptions were solicited for placing him to be educated under the inspection and care of several mathematical gentlemen: but I have been informed, that the plan was relinquished, from some reasons on the part of his father; and he is again to be seen by the public. A subscription is now solicited for publishing a portrait of him on the following terms.]

Remarkable powers of computation in a child.

ZERAH COLBURN, a child *just eight years of age*, without any *previous* knowledge of the common rules of arithmetic, or even of the *use and power* of the Arabic numerals, and without having given any particular attention to the subject, possesses (as if by intuition) the singular faculty of solving a great variety of arithmetical questions *by the mere operation of the mind*, and without the usual assistance of any visible symbol or contrivance.

This print will be engraved from a drawing by Mr. Trumbull; and the size of it will be about 12 inches by 10.

The price to subscribers will be One Guinea, to be paid at the time of subscribing: and the plates will be delivered according to the order of subscription.

The following gentlemen (who are well acquainted with the extra-

Remarkable powers of computation in a child.

extraordinary abilities of this child) have kindly undertaken to attend to the progress and execution of the work, and to see to the distribution of the plates, viz. Sir James Mackintosh; Dr. W. H. Wollaston, Sec. R. S.; William Vaughan, Esq.; John Bonnycastle, Esq., Math. Prof.; Francis Wakefield, Esq.; William Allen, Esq., F. R. S. F. L. S.; John Guillemard, Esq., F. R. S. F. Amer. S.; Samuel Parker, Esq.; Francis Bailey, Esq.

Subscriptions are received by either of the above gentlemen, or by Messrs. Johnson and Co., No. 72, St. Paul's churchyard: and printed receipts will be given for the same, which must be produced and given up at the time the plates are delivered.

Zerah Colburn is at present to be seen at the Exhibition Rooms, Spring Gardens. Many persons of the first eminence for their knowledge in mathematics, and well known for their philosophical inquiries, have made a point of visiting him: and they have all been struck with astonishment at his extraordinary powers. It is correctly true, as stated of him, that—"He will "not only determine, with the greatest facility and dispatch, the "exact number of *minutes* or *seconds* in any given period of time; "but will also solve any other question of a similar kind. He "will tell the exact *product* arising from the multiplication of "any number, consisting of two, three, or four figures, by any "other number consisting of the like number of figures. Or, "any number, consisting of six or seven places of figures, "being proposed, he will determine, with equal expedition and "ease, *all* the *factors* of which it is composed. This singular "faculty consequently extends not only to the *raising of powers*, "but also to the extraction of the *square* and *cube roots* of the "number proposed; and likewise to the means of determining "whether it be a *prime* number (or a number incapable of division by any other number); for which case there does not "exist, at present, any general rule amongst mathematicians." All these, and a variety of other questions connected therewith, are answered by this child with such *promptness* and *accuracy* (and in the midst of his juvenile pursuits) as to astonish every person who has visited him.

At a meeting of his friends, which was held for the purpose of concerting the best method of promoting the views of the father respecting his education, this child undertook, and completely

Remarkable powers of computation in a child.

pletely succeeded in, raising the number 8 *progressively* up to the *sixteenth* power; and in naming the last result, viz. 281,474,976,710,656, he was right in every figure. He was then tried as to other numbers, consisting of one figure; all of which he raised (by actual multiplication and not by memory) as high as the *tenth* power: with so much facility and dispatch, that the person appointed to take down the results was obliged to enjoin him not to be so rapid. With respect to numbers consisting of two figures, he would raise some of them to the *sixth, seventh*, and *eighth* power; but not always with equal facility: for the larger the products became, the more difficult he found it to proceed. He was asked the *square root* of 106929, and before the number could be written down, he *immediately* answered 327. He was then required to name the *cube root* of 268,336,125, and with equal facility and promptness he replied 645. Various other questions of a similar nature, respecting the roots and powers of very high numbers, were proposed by several of the gentlemen present, to all of which he answered in a similar manner. One of the party requested him to name the *factors* which produced the number 247483, which he immediately did by mentioning the two numbers 941 and 263; which indeed are the only two numbers that will produce it. Another of them proposed 171395, and he named the following factors as the only ones that would produce it; viz. 5 × 34279, 7 × 24485, 59 × 2905, 83 × 2065, 35 × 4897, 295 × 581, and 413 × 415. He was then asked to give the factors of 36083; but he immediately replied that it had none; which in fact was the case, as 36083 is a prime number[*]. Other numbers were indiscriminately proposed to him, and he always succeeded in giving the correct factors, except in the case of prime numbers, which he discovered almost as soon as proposed. One of the gentlemen asked him how many *minutes* there were in forty-eight years; and before the question could be written down, he replied 25,228,800; and instantly added, that the number of

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[*] It had been asserted and maintained by the French mathematicians, that 4,294,967,297 (= $2^{32} + 1$) was a *prime* number: but the celebrated Euler detected that errour by discovering, that it was equal to 6,700,417 × 641. The same number was proposed to this child, who found out the factors by the mere operation of his mind.

seconds

Remarkable powers of computation in a child.

seconds in the same period was 1,513,728,000. Various questions of the like kind were put to him; and to all of them he answered with nearly equal facility and promptitude; so as to astonish every one present, and to excite a desire that so extraordinary a faculty should (if possible) be rendered more extensive and useful.

It was the wish of the gentlemen present to obtain a knowledge of the method by which the child was enabled to answer, with so much facility and correctness, the questions thus put to him: but to all their inquiries upon this subject (and he was closely examined upon this point) he was *unable* to give them any information. He positively declared (and every observation that was made seemed to justify the assertion) that he did not know *how* the answers came into his mind. In the act of multiplying two numbers together, and in the raising of powers, it was evident (not only from the motion of his lips, but also from some singular facts which afterward occurred,) that some *operation* was going forward in his mind; yet that could not (from the readiness with which the answers were furnished) be at all allied to the usual mode of proceeding with such subjects: and moreover, he is entirely ignorant of the common rules of arithmetic, and cannot perform, upon paper, a simple sum in multiplication or division. But, in the extraction of roots and in mentioning the factors of high numbers it does not appear that any operation *can* take place; since he will give the answer *immediately*, or in a very few *seconds*, where it would require, according to the ordinary method of solution, a very difficult and laborous calculation: and moreover, the knowedge of a *prime* number cannot be obtained by any *known* rule.

It may naturally be expected, that these wonderful talents, which are so conspicuous at this early age, will by a suitable education be considerably *improved* and *extended*; and that some *new* light will eventually be thrown upon those subjects, for the elucidation of which his mind appears to be peculiarly formed by nature, since he *enters into the world* with all those powers and faculties, which are not even attainable by the most eminent *at a more advanced period of life*. Every mathematician must be aware of the important advantages, which have sometimes been derived from the most simple and trifling circumstances; the full effect of which has not always been evident at first sight.

Remarkable powers of computation in a child.

sight. To mention one singular instance of this kind. The very simple improvement of expressing the powers and roots of quantities by means of *indices* introduced a new and general *arithmetic of exponents*; and this algorithm of powers led the way to the *invention of logarithms*, by means of which all arithmetical computations are so much facilitated and abridged. Perhaps this child possesses a knowledge of some *more important* properties connected with this subject; and although he is incapable at present of giving any satisfactory account of the state of his mind, or of communicating to others the knowledge which it is so evident he *does* possess, yet there is every reason to believe, that, when his mind is more cultivated and his ideas more expanded, he will be able not only to divulge the mode by which he at present operates, but also point out some *new sources* of information on this interesting subject.

The profits of the present print will be given to the father of this child, in order to enable him to provide a more *suitable* education for his son: and it is hoped that the friends of science, and the public in general, will promote a plan, which promises to be attended with such advantages.

III.

Farther Experiments and Observations on the Action of Poisons on the Animal System. By B. C. Brodie, *Esq. F. R. S. Communicated to the Society for the improvement of Animal Chemistry, and by them to the Royal Society.*

(*Concluded from p.* 268.)

IV. Experiments with the Muriate of Barytes.

Barytes poisonous, but less so than its salts.

WHEN barytes is taken into the stomach, or applied to a wound, it is capable of destroying life; but when in its uncombined state its action is very slow. The muriate of barytes, which is much more soluble than the pure earth, is (probably on this account) a much more active poison.

Exp. 5. Mu-

Experiment 5. Ten grains of muriate of barytes rubbed very riate of barytes applied to a wound in a rabbit.

fine, and moistened with two drops of water, were applied to two wounds in the thigh and side of a rabbit. In four minutes he was evidently under the influence of the poison. In a short time he became giddy: then his hind legs were paralysed; and he gradually fell into a state of insensibility, with dilated pupils, and lay in general motionless, but with occasional convulsions. The pulse beat 150 in a minute, but feeble; and it occasionally intermitted. He was apparently dead in twenty minutes from the application of the poison; but on opening the chest, the heart was found still acting, and nearly three minutes elapsed before its action had entirely ceased.

Exp. 6. Solution of muriate of barytes injected into the stomach of a cat.

Experiment 6. An ounce and a half of saturated solution of muriate of barytes were injected into the stomach of a full grown cat, by means of an elastic gum tube. In a few minutes it operated as an emetic. The animal became giddy, afterward insensible, and lay with dilated pupils, in general motionless, but with occasional convulsions. At the end of sixty-five minutes, from the beginning of the experiment, he was apparently dead; but the heart was still felt through the ribs acting one hundred times in a minute. A tube was introduced into the trachea, and the lungs were inflated about thirty-six times in a minute; but the pulse sunk notwithstanding, and at the end of seven minutes the circulation had entirely ceased.

It appears to act chiefly on the brain:

but in some degree on the heart.

From these experiments I was led to conclude, that the principal action of the muriate of barytes is on the brain; but in the first the pulse was feeble and intermitting; in the second, although the artificial respiration was made with the greatest care, the circulation could not be maintained more than a few minutes. These circumstances led me to suspect, that although this poison operates principally on the brain, it operates, in some degree, on the heart also. Farther experiments confirmed this suspicion. In some of them the pulse soon became so feeble, that it could be scarcely felt; and its intermissions were more frequent; but in all cases the heart continued to act after respiration had ceased; and the cessation of the functions of the brain was therefore always the immediate cause of death. When I employed artificial respiration, after death had apparently taken place, I seldom was able to prolong the heart's action beyond a few minutes. In one case only it was maintained for three quarters of an hour. I never by these means succeeded in restoring the animal to life, although the experiments were made with the greatest care, and in a warm temperature. In some instances, after the artificial respiration had been kept up for some time, there were signs of the functions of the brain being in some degree restored; but the pulse notwithstanding continued to diminish in strength and frequency, and ultimately ceased. I shall detail one of these experiments, as it serves to illustrate the double action of this poison on the nervous and vascular systems.

Exp. 7. Action of the muriate of barytes on the nervous and vascular system illustrated.

Experiment 7. Some muriate of barytes was applied to a wound in the side of a rabbit. The usual symptoms took place, and at the end of an hour the animal was apparently dead; but the heart still continued to contract. He was placed in a temperature of 80°, and a tube being introduced into the nostril, the lungs were artificially inflated about thirty-six times in a minute.

When the artificial respiration had been maintained for four minutes, he appeared to be recovering; he breathed voluntarily one hundred times in a minute, and showed signs of sensibility. The artificial respiration was discontinued. The voluntary respiration continued about nine minutes, when it had ceased, and the animal was again apparently dead; but the pulse continued strong and frequent. The lungs were again artificially inflated. At the end of four minutes the animal once more breathed voluntarily one hundred times in a minute, and repeatedly moved his limbs and eyelids. The pulse became slower and more feeble.

In a few minutes the voluntary respiration again ceased, and the artificial respiration was resumed. The pulse had fallen to one hundred, and was feeble. The animal again breathed voluntarily; but he ceased to do so at the end of five minutes. The lungs were inflated as before; but he did not give any sign of life, nor was the pulse felt afterward. On opening the thorax, his heart was found to have entirely ceased acting.

Like arsenic, it renders the

A probe having been introduced into the spinal marrow, it was found, that by means of the Voltaic battery powerful contractions might be excited, not only of the voluntary muscles, but also of the heart and intestines; from which it may be inferred, that the muriate of barytes, like arsenic, affects the

circuheart insensible to the stimulus of the blood.

circulation by rendering the heart insensible to the stimulus of the blood, and not by destroying altogether the power of muscular contraction.

It affects the stomach, but less than arsenic.

The muriate of barytes affects the stomach, but in a less degree than arsenic. It operates as an emetic in animals that are capable of vomiting; but sooner when taken internally, than when applied to a wound. In general, but not constantly, there are marks of inflammation of the inner membrane of the stomach, but not of the intestine. In many instances there is a thin layer of dark coloured coagulum of blood lining the whole inner surface of the stomach, and adhering very closely to it, so as to have a good deal of the appearance of a slough; and this is independent of vomiting, as, where I met with it, it occurred in rabbits.

The same circumstances, from which it may be inferred, that arsenic does not produce its deleterious effects until it has passed into the circulation, leads to the same conclusion with regard to the muriate of barytes.

V. *On the Effects of the Emetic Tartar.*

Emetic tartar has similar effects.

The effects of the emetic tartar so much resemble those of arsenic and of muriate of barytes in essential circumstances, that it would be needless to enter into a detail of the individual experiments made with this poison.

Applied to a wound.

When applied to a wound in animals, which are capable of vomiting, it usually, but not constantly, operates very speedily as an emetic; otherwise I have found no material difference in the symptoms produced in the different species of animals, which I have been in the habit of employing as the subjects of experiment. The symptoms are paralysis, drowsiness, and at last complete insensibility; the pulse becomes feeble; the heart continues to act after apparent death; its action may be maintained by means of artificial respiration, but never for a longer period than a few minutes: so that it appears, that this poison acts on the heart as well as on the brain; but that its principal action is on the latter. Both the voluntary and involuntary muscles may be made to contract after death, by means of Voltaic electricity. The stomach sometimes bears the marks of inflammation; but at other times it has its natural appearance. I have never seen any appearance of inflammation of the intestines. The length of time which elapses from the application of the poison to the death of the animal varies. In some instances it is not more than three quarters of an hour; but in others it is two or three hours, or even longer.

Acts in the same manner internally.

When a solution of emetic tartar was injected into the stomach of a rabbit, the same symptoms took place as when it was applied to a wound.

VI. *On the Effects of the Corrosive Sublimate.*

Effects of muriate of mercury.

When this poison is taken internally in very small and repeated doses, it is absorbed into the circulation, and produces on the system those peculiar effects, which are produced by other preparations of mercury. If it passes into the circulation in larger quantity, it excites inflammation of some part of the alimentary canal, the termination of which may vary accordingly as it exists in a greater or less degree. When taken in a larger quantity still, it occasions death in a very short space of time. I had found, that, if applied to a wounded surface, it produced a slough of the part to which it was applied, without occasioning any affection of the general system. This led me to conclude, that the effects of it, taken internally, and in a large quantity, depended on its local action on the stomach, and were not connected with the absorption of it into the circulation. The following experiments appear to confirm this opinion.

They depend on its local action.

Exp. 8. Administered internally to a rabbit.

Experiment 8. Six grains of corrosive sublimate, dissolved in six drams of distilled water, were injected into the stomach of a rabbit, by means of an elastic gum tube. No immediate symptoms followed the injection; the animal made no expression of pain; but in three minutes he became insensible; was convulsed; and in four minutes and a half from the time of the injection being made, he died. Tremulous contractions of the voluntary muscles continued for some time afterward. On opening the thorax, the heart was found to have entirely ceased acting, and the blood in the cavities of the left side was of a scarlet colour. The stomach was much distended. The pyloric and cardiac portions were separated from each other by a strong muscular contraction. The contents of the former were firm and solid, and in every respect resembled the usual contents of the stomach; while those of the cardiac portion consisted of the food of the animal much diluted by fluid; so that the solution, which had been injected, appeared to be confined to the cardiac portion of the stomach, and to be prevented entering the pyloric portion by the muscular contraction in the centre.

In the pyloric portion of the stomach the mucous membrane had its natural appearance; but in the cardiac portion it was of a dark gray colour, was readily torn and peeled off; and in some parts its texture was completely destroyed, so that it appeared like a pulp, on removing which the muscular and peritoneal coats were exposed.

Experiment repeated.

The repetition of the experiment was attended with similar results. The alteration of the texture of the internal membrane appears to have been occasioned by its being chemically acted on by the corrosive sublimate injected into it. When the injection is made into the stomach of a dead rabbit, precisely the same effects are produced, except that, as the middle contraction is here wanting, the appearances are not confined in the same degree to the cardiac portion.

Similar effects on the stomach of a dead rabbit.

Exp. 9. Muriate of mercury given to a cat.

Experiment 9. A scruple of corrosive sublimate, dissolved in six drams of distilled water, was injected into the stomach of a full grown cat. For the first five minutes no symptoms were produced. After this, the poison operated twice as an emetic. The animal appeared restless, and made expression of pain in the abdomen. He gradually became insensible, and lay on one side motionless, with the pupils of the eyes dilated. The respiration was laborious, and the pulse could not be felt. Twenty-five minutes after the poison was injected, there was a convulsive action of the voluntary muscles, and death ensued. On opening the thorax immediately afterward, the heart was seen still contracting, but very feebly.

Appearances on dissection.

The stomach was found perfectly empty and contracted. The mucous membrane was every where of a dark gray colour. It had lost its natural texture, and was readily torn and separated from the muscular coat. The internal membrane of the duodenum had a similar appearance, but in a less degree, for nearly three inches from the pylorus. In the situation of the pylorus the effects of the poison were less apparent than in any other part.

The particular state of the internal membrane of the stomach, in this experiment as well as in the last, appears to have been occasioned by the chemical action of the poison on it. When I injected a solution of corrosive sublimate into the stomach of a dead cat, and retained it there for a few minutes, a similar alteration of the texture of the internal membrane took place; but it assumed a lighter gray colour. The difference of colour may be explained by the vessels in the one case being empty, and in the other case being distended with blood at the time of the injection being made.

Effects on the stomach of a dead cat similar.

The destruction of the substance of the internal membrane of the stomach precludes the idea of the poison having been absorbed into the circulation. We must conclude, that death was the consequence of the chemical action of the poison on the stomach. This organ, however, is not directly necessary to life, since its functions, under certain circumstances, are suspended for hours, or even for days, without death being produced. Although the stomach was the part primarily affected, the immediate cause of death must be looked for in the cessation of the functions of one or more of those organs, the constant action of which is necessary to life. From the scarlet colour of the blood in the left side of the heart, in the experiment on the rabbit, we may conclude, that the functions of the lungs were not affected; but the affection of the heart and brain is proved by the convulsions, the insensibility, the affection of the pulse in both experiments, and the sudden cessation of the heart's action in the first; and we may therefore be justified in concluding, that the immediate cause of death was in both of these organs. As the effects produced appear to have been independent of absorption, we may presume, that the heart, as well as the brain, was acted on through the medium of the nerves.

The muriate acts directly on the stomach;

but produces death by indirectly destroying the functions of the heart and brain.

That a sudden and violent injury of the stomach should be capable of thus speedily proving fatal, is not surprising, when we consider the powerful sympathy between it and the organs on which life more immediately depends, and the existence of which many circumstances in disease daily demonstrate to us.

VII. The facts which have been stated appear to lead to the

General infe-

following

rences respecting the action of these mineral poisons.

following conclusions respecting the action of the mineral poisons, which were employed in the foregoing experiments.

1. Arsenic, the emetic tartar, and the muriate of barytes, do not produce their deleterious effects until they have passed into the circulation.

2. All of these poisons occasion disorder of the functions of the heart, brain, and alimentary canal; but they do not all affect these organs in the same relative degree.

3. Arsenic operates on the alimentary canal in a greater degree than either the emetic tartar, or the muriate of barytes. The heart is affected more by arsenic than by the emetic tartar, and more by this last, than by the muriate of barytes.

4. The corrosive sublimate, when taken internally in large quantity, occasions death by acting chemically on the mucous membrane of the stomach, so as to destroy its texture; the organs more immediately necessary to life being affected in consequence of their sympathy with the stomach.

Mineral and vegetable poisons compared.

In making the comparison between them, we observe, that the effects of mineral, are less simple than those of the generality of vegetable poisons; and when once an animal is affected by the former, there is much less chance of his recovery, than when he is affected by the latter.

IV.

IV. *On the Vegetation of high Mountains, translated from a Paper of Mr.* Ramond's *in the Annales du Museum, V. 4, p.* 395. *By* Richard Anthony Salisbury, *Esq. F. R. S.* &c*.

Vigorous vegetation on mountains in temperate regions.

An observing gardener, on ascending the high mountains of our temperate region, is immediately struck with the vigour and luxurious appearance of their vegetation. The plants he has seen in the adjacent plains are changed in size, aspect, and form, so that he hardly recognises the most common. Their stems are elevated, their flowers larger, even the leaves of the trees have acquired a size, which makes him doubt the identity of the species. The woods are more impenetrable, the turf of the downs closer, and a green more lively, fresh, and brilliant, colours every thing, from the depths of the valley, up to those heights, where the eye can discern nothing but naked rocks and eternal snows*.

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* Hort. Trans. vol. I, appendix, p. 15.

Growth of vegetables rapid.

Thus, endowed with a vigour elsewhere unknown, vegetables there hasten with increased energy through the various periods of their existence. Time, which to them moves slowly in the plains, in the mountains flies. There, every thing is done rapidly; meteors dart after each other, and the air is in perpetual agitation. From all these controlling causes, acting together in full force, germination, florescence, and fructification take place almost simultaneously. Sometimes, with a wind blowing from the south, with a heavy shower, or with a scorching sun, the face of the meadows, downs, and forests, in a moment changes, and the whole of a particular species seems to vanish; in fact, there, every fine day is a spring to some particular assemblage of vegetables, or to some of the inaccessible heights in which they grow.

Their localities more distinct.

To this picture, another succeeds. If we examine the mountains and vallies, every place has its peculiar soil, every different elevation its peculiar climate, and each of them its characteristic vegetables. In the plains, these vegetable assemblages occupy vast spaces, the limits of which are too extensive, and indeterminate, to be easily perceived. On the contrary, in the mountains, they are confined to narrow limits, which the eye often takes in at one view. In a gentle rising extended between two dales, in a pile of rocks, or in a cliff, which the traveller ascends in a few moments, he finds the perpetual barriers of those productions, which nature has been pleased to separate.

Particular effect of height.

Among the various causes of these separations, one seems to reign predominant over all others; this is, elevation above the level of the sea. In every 100 inches in height, the temperature falls about half a degree of our thermometers. After that degree of cold, which generally puts a stop to all vegetation, an eternal frost prevails on the summit of these Alps, as at the poles, and every 100 metres of vertical elevation, corresponds nearly to one degree of the distance at which the mountain is placed from the pole.

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* The first part of this sentence rather applies to purely mountainous plants, such as *aster alpinus*, *viola grandiflora*, *aquilegia vulgaris*, &c., than to all vegetables indiscriminately; the latter part I should explain by saying, that the foliage of the trees was rather diminished in the dry plains at the base of the *Pyrenees*, than enlarged by mere elevation, but, along with elevation, to a certain extent, perpetual moisture and food are washed down to their roots; and such a situation in *France*, is probably the aboriginal one of the trees in question. *Sec.*

100 yards nearly equivalent to a degree of latitude.

Two causes of the distribution of vegetables.

By this scale, the various phenomena of different climates in our globe may be easily understood; circumstances may differ, but the general results will be nearly the same. While the increase of cold is accompanied by a diminution of the column of air, it is also affected by the obliquity of the rays of the sun, and the distribution of vegetables, in all alpine countries, depends principally on these two causes.

Trees.

Thus, in the *Swiss Alps*, and *Pyrenees*, trees cease to grow at about 2400 or 2500 metres of actual elevation, as they do about the 70th degree of north latitude; and that circle these gigantic vegetables occupy, is divided into several less bounds, which have each their peculiar characteristics. At the foot of the mountain we find the *oak*: in the middle region the *beech*: above these the *fir* and *yew* succeed, which soon give place to the *pine* (Pinus *sylvestris* L.). Along with this last mentioned tree, in the *Swiss Alps* the *larch* and *cembro* (Pinus *cembra* L.) also grow wild, which are unknown in the *Pyrenees*. The *cedar of Lebanus* would probably thrive as well on these mountains, as on those of Asia, had it been fixed there; but such is still the mystery of the original dissemination of vegetables, that Nature seems by turns, indifferent to the similitude of places, or to the distance between them; sometimes bringing together in the same climate, plants of the most distant countries; and sometimes denying this conformity of vegetables to regions exactly alike, both in soil and temperature.

Rhododendron.

In this zone of trees, the *rhododendron ferrugineum* L. a little shrub peculiar to the mountains of *Europe* solely, is very abundant. It never descends into the plains, and can hardly be cultivated in a garden, demanding its native air, soil, water, nay snows, and even there only occupies particular spots. Nothing is more beautiful when in flower, but nothing is more untractable. In the *Pyrenees* it first appears at exactly 1600 metres of elevation, stopping as precisely at 2600 metres, and within these limits, is so abundant and vigorous, that it would be as difficult to extirpate it there, as it is to cultivate it elsewhere*.

Juniper.

The *juniper* traverses far beyond this circle, up to the elevation of 2900 metres, but this shrub, as it ascends, gradually loses the habit and appearance, which distinguish it in our plains: there, it resembles the *juniper* of *Sweden* and *Lapland*, with a low spreading stem, prostrate on the ground, seeking an asylum, as it were, by instinct on those sides of the rocks exposed to the south or west, against which it spreads out its branches into an espalier, with a regularity which art can seldom attain†.

Annuals scarcely found at a certain height.

In a more elevated region, we find the rigour of the climate will not permit the existence of any shrub whatever, which the first snows do not entirely cover. Still higher, even this shelter is insufficient, and nothing but a few herbs, with perennial roots actually under the earth, subsist. Nature has almost entirely banished from such places annual plants; where the whole summer is reduced to a few days, nay, sometimes a few hours; where often a storm of wind, or dripping fog, will destroy the flowers which have scarcely blossomed, and, bringing back winter, terminate the year.

Hardy perennials.

On the contrary, hardly any elevation seems to stop the progress of some perennials, which, on the approach of severe cold, shelter themselves under the double protection of the earth and snow, forming their buds underground, and springing up the first fine day of the succeeding year. Their duration exhausts the chances of all times and seasons, till, sooner or later, they also ripen seed, by which they are multiplied.

Plants at the height of 3273 yards,

Thus the vegetable zone of our alps has in fact no other limits, than those of the earth or soil covering them. The *Pic du Midi*, which I have ascended 26 times, is 3000 metres above the level of the sea, but I never once found the thermometer there rise to the temperate point. Yet, on a nearly bare rock, I have there gathered as many as 48 species of vegetables, excluding cryptogamous plants: of these, one only, which perhaps I may never

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* No shrub is more plentiful, or easily cultivated in the gardens about *London*, if planted in light sandy peat under a rock, or north-west wall, and watered plentifully in dry weather.—*Sec.*

† Two distinct species are probably here confounded, an opinion in which I was confirmed by the late Mr. Dryander.—*Sec.*

find

at 3552 yards. find again, was annual. At *Nieuville*, a place 250 metres higher than the *Pic du Midi*, where the thermometer in summer never rises to more than 8 degrees, I have, in five journies, collected 12 different perennials. at 3825 yards. On the top of *Mont Perdu*, at an elevation of 3500 metres, even in the bosom of permanent snows, but on rocks the sloping situation of which had cleared them of snow, I have seen six different plants very vigorous. Here, in one of the hottest days of a summer remarkable for its heat, the thermometer only rose to 5·5° above the point of congelation, and it undoubtedly falls in winter to 25 or 30: nor is it certain, that those 6 plants, found in a season which melted more snow than usual, are regularly uncovered every year. Besides, I have seen some of them on the borders of the perpetual snow, with only half of their stems exposed and vegetating, the other half buried in it*, and it is probable, that many of them do not see the light ten times in a century, running through the whole course of their vegetation in a few short weeks, and doomed afterwards to sleep through a winter of many years.

These plants confined to mountains, or the vicinity of the poles.

Plants subjected to so singular a mode of existence are not among the species which grow in the plains of our temperate regions: they belong exclusively to such as grow on the summits of mountains, or near the poles. *Norway*, *Lapland*, and *Greenland*, furnish plants analogous to those of the *Swiss Alps* and *Pyrenees*; but few, or possibly none of them, are seen in *Siberia*, *Kamschatka*, or even in the polar regions of *America*. One would hardly have supposed so great a diversity of vegetable productions in countries so much alike and near each other, nor on the other hand, so great a conformity as exists among the plants of these countries, and the plants of some alpine regions distant from them 40 degrees.

Plants not disseminated in parallel latitudes.

In fact, we learn from actual observation, that the dissemination of vegetables is not always regulated in parallel distances from the equator; that if a certain number of plants, confined by their constitution to a peculiar climate, are to be found to a certain distance under the same latitudes, many others, on the contrary, have been scattered over different countries in the direction of their meridians. Towards the south, *America*, *Africa*, and *Asia*; towards the north, *Europe*, *Asia*, and *America*, are far from producing the same vegetables under the same parallels; while many plants, growing wild in each of these grand divisions of the globe, brave every obstacle opposed to them by a diversity of climate, and propagate themselves in a geographical direction quite contrary to that which a similar climate would confine them to.

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* A similar case occurred in a vine at *Chapel Allerton*, planted in the open air, at some distance from the stove; a branch of which, however, being introduced into the stove early in *January*, was loaded with clusters of grapes, before any of the buds exposed to the open air, shot out.—*See.*

Progress of curious plants.

Thus, for example, many of the curious plants of *Sardinia*, *Sicily*, and *Italy*, mount up the *Swiss Alps*, and then descend again into the lower parts of *Germany*, without being allured by our fine climate to *France*. Thus, likewise, the *Pyrenees* receive from *Spain* a great number of the plants of *Barbary*, scattering them over the western provinces of *France*. The *merendera*, which grows in the north of *Africa*, is found in *Andalusia*, *Castile*, *Arragon*; when crossing the *Pyrenees* it descends as far as the *Landes de Bourdeaux*. The *narcissus bulbocodium**, and *hyacinthus serotinus*, grow wild in the same places, and follow the same route. The *anthericum bicolorum* of *Algiers*, traverses the same chain of mountains, and arrives in *Anjou*. The *scilla umbellata* and *crocus nudiflorus*, have migrated from the *Pyrenees* even into *England*. Yet not one of the above mentioned vegetables have been disseminated laterally, to meet those southern ones which have crossed the *Swiss Alps*.

This most striking in the valleys of the Pyrenees.

But it is in the great valleys of the *Pyrenees*, extending from north to south, that these vegetable galaxies become most striking and singular. The *dianthus superbus* runs through the whole valley of *Campan* and *Gavarnie*, without ever entering any

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Three species confounded by the author.

* Here the celebrated author confounds three very distinct species. The plant of the *Pyrenees* is the *N. Bulbocodium* L. with erect leaves, very hardy, and brought forced to *Covent-garden* in abundance every spring. The plant of *Barbary* and *Andalusia*, which I received from the late professor Broussonet, is more dwarfish, with leaves spreading flat on the ground, and so tender, that it will only live here through winter, in very warm sandy soils, close to a wall. The plant of *Castile* grows also near *Oporto*, and differs from both the others, in having a six-lobed plaited crown, with very narrow leaves: it is not very tender, but requires a dry sandy soil. *&c.*

Verbascum Myconi.

of the side ones. The *verbascum Myconi*, that beautiful and scarce plant, which does not belong either to the genus in which LINNEUS has placed it, or perhaps to any natural order yet defined, and which has so exotic an appearance, that it distinguishes itself like the *king fisher*, among our indigenous birds, invariably keeps to the same direction. Nothing is more abundant in all the great valleys of the *Pyrenees*, in every soil and exposition: yet the very same soil and exposition never attract it to any of the collateral ones. I could cite a multitude of similar examples, but it is sufficient at present, to mention one more, the *box tree*. This shrub, so very robust, is affected by elevation like the most delicate ones. At the base of the *Pyrenees*, both on the *French* and *Spanish* side, it covers every hill: thence it enters the great valleys, running from the north-east towards the south, but never quits them; in vain do the numerous branches of these valleys offer it an asylum; passing their openings, it keeps to its first direction, stopping on the crest of the chain at about 2000 metres above the level of the sea, and appearing again on the other side at a similar elevation, and in a similar direction, from which it never deviates.

Box.

Local influence striking in mountainous countries,

Thus it is, that in high mountainous countries we discover the strongest traces of the original design of nature; there, each order of vegetables is confined within narrower bounds; there, local influence more powerfully resists every other. Nevertheless, the lapse of ages, and especially the presence of man, has here introduced many modifications; for, in traversing the immense deserts of these high mountains, among the rare plants which form their herbage, some few of the commonest here and there occur. If the verdure takes a deeper tint than usual, contrasted with the gayer colour of the alpine turf, the ruins of a hut, or a rock blackened by smoke, explain the mystery. Around these asylums of man, we find naturalized the common *mallow*, *nettle*, *chickweed*, common *dock*. A shepherd had possibly sojourned here some weeks, and, hither, in driving his flocks here, had also attracted without knowing it, the birds, the insects, the seeds of the plants of his lowland cot. He may possibly never return, but these wild spots have received in an instant the indelible impression of

but even here modified by man.

of his footsteps; so much weight has a being of his importance in the scale of nature.

Woods destroyed by him.

In other places, by destruction he has signalized his presence. Before he approached the mountains, the immense forests which covered their bases have fallen under his axe, for woods are not the abodes of man; he avoids the circuitous paths of so vast a labyrinth, suspecting danger under their shades; he there mourns the absent sun, an object which every day renovates his delight; and therefore it is seldom that he penetrates a forest, without fire and sword in hand.

and with them woodland plants.

Accordingly the seeds of woodland plants become dormant in a soil now dried by the sun and wind, and no longer suitable to their germinating. Other vegetables take their places, the climate itself changing; for the temperature rises, the rains are less frequent, but more copious, the winds more inconstant and impetuous, deep gullies are formed in the sides of the acclivities by torrents, and rocks are deprived of the earth which covered them, and, at the same time, of the plants which ornamented them, by falls of immense loads of melting snow; thus the face of the globe, where man inhabits, is more changed in one century, than in twenty where he is absent.

The horticulturist should visit alpine countries as well as the geologist.

After all, in Alpine countries, the different soils, and their productions, retain most of their aboriginal character: there, the primitive distribution of vegetables has been least disturbed; their localities can be easily traced, the influence of the air is most perceptible; there, the contiguity of objects exhibiting more forcibly their similitudes and dissimilitudes, the eye of the observer takes in, at one glance, every trait which is interesting; and if it is necessary for the geologist to visit these grand chains of mountains, to study the structure of the earth and those catastrophes, which have imprinted its present form, it is still more so for the horticulturist, who wishes to penetrate the mysteries of the primary dissemination of vegetables and their subsequent propagation, hoping thence to derive hints for their successful cultivation and improvement, in the paradise surrounding his dwelling.

IV.

Description of a Bank for Alpine *Plants, by Monsieur* THOUIN, *abridged from his Paper in the* Annales du Museum, *V.* 6, p. 183. *By* RICHARD ANTHONY SALISBURY, *Esq. F. R. S.* &c.*

Bank for the culture of alpine plants, in the botanical garden at Paris.

PLANTS from alpine and frozen countries are cultivated in the *Jardin des Plantes* at *Paris*, in a *bank*, 60 feet long, placed against the wall of a terrace, 10 feet high, which faces the south-east so much, that the sun ceases to shine upon it between 10 and 11, A. M. This bank is divided into 5 steps, 1 foot wide, by nailing planks of oak, 10 inches deep, to the top of as many rows of strong posts, charred at the bottom, and driven firmly into the ground; the taller posts are still further secured in their places by cross bars let into the wall.

Through the whole length of this *bank* runs a ditch, 2 feet deep, but sloping gradually towards the front up to 9 inches in height, under the general level of the ground; and in making this ditch, its sides were plastered 6 inches thick with mortar of brick mould and chopped straw; filling up all the cracks which appeared during the week it was left exposed to the air. After nailing the planks to the posts, the natural soil, which is of a light nature, was thrown into the hollow up to within about a foot of the surface of the slope, above which it was filled with sandy peat, such as *ling* and *heaths* grow in, passed through a screen. My reason for using all these precautions was to prevent the water necessary for the health of those *alpine* plants in summer, running off too quickly into a bed of dry gravel underneath; in a naturally moist soil, this expense and trouble may be saved.

Seeds sown in it.

I have sown on this *bank* the seeds received not only from the *Alps*, but several other frozen regions; for it is probable, that the elevation of the atmosphere near the *poles* corresponds with that of the highest mountains in *France*, rising gradually toward the equator; nor is this consideration so foreign to the business of a gardener in naturalizing vegetables, as might be at first supposed.

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* Horti. Trans. vol. I, appendix, p. 24.

Roots planted in it.

Roots of all the *alpine* plants I could collect, have also been planted in this *bank*, and they thrive much better than when cultivated in pots on a stage, however open or airy, so that most of the following have greatly increased both by seeds and roots. *Moehringia muscosa, viola biflora, androsace carnea,* and *lactea, soldanella alpina, primula farinosa,* tussilago, alpina, artemisia glacialis, salix myrsinites, retusa,* and *reticulata.*

Catalogue.

Management.

The culture they require is, 1st, to keep the *bank* carefully weeded: 2dly, to reduce within bounds many that grow and spread rapidly so as to exclude others: 3dly, to dig and lighten the surface frequently, that it may absorb air and water more readily: 4thly, to add three inches in depth of fresh sandy peat every year, in place of the old, which soon loses its *humus*, or nutritious part: 5thly, in giving the plants, at a certain season, not only daily, but hourly waterings; but this being one of the most important points, I shall enlarge more fully upon it.

Alpine plants naturally watered by the melting snow.

Almost all *alpine* plants are of humble stature, growing on steep declivities of rocks in a layer of *humus* or vegetable earth, formed by the decomposition of *jungermannias, lichens,* and *mosses.* The greater part of the year, they are covered with a bed of snow, which only begins to melt at stated periods of the day, after the rays of the sun have acquired great force. Then only do these *alpine* plants awaken from torpidity, exhaling quickly in this light black soil the moisture which they have absorbed during the night; but the returning sun, which excites them to action, also melts the snow above, the waters of which trickling down to their roots, give immediate refreshment. The sun disappearing, these little vegetables are no longer exhausted, and a continuance of moisture would even be hurtful; accordingly the snow resuming its solid consistence with the cold of the night, this natural irrigation ceases, with a degree of exactness, that the most careful gardener cannot perform.

Artificial watering.

From the above remarks, it will easily be deduced, that *alpine* plants should have no water at all during winter and dank moist weather: on the contrary, that they should be kept perpetually

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* I have constantly found this plant growing wild in wet meadows that are seldom dry even in summer, at the foot of the mountains, and even in bogs. *See,*

petually moist during hot sunshine, by water dribbling through the soil to their roots, without wetting their leaves, which, immediately evaporating by the heat, will cool the air just above them. In fact, it is only by a close imitation of the process of nature, that these vegetables of cold regions can be successfully cultivated in botanic gardens.

They must be covered from frost.

The last essential point relative to *alpine* plants is to cover them up on the approach of frost: this may appear a strange precaution to some, but when winter commences in their native soil, being immediately covered with snow to the depth of seven inches, they never feel a greater degree of cold than that of the freezing point, the soil itself being hardly frozen. The best covering is that of *fern, pteris aquilina*, which does not absorb moisture so quickly as most other sorts of haulm.

V.

On a Periscopic Camera Obscura and Microscope. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S. From the Philosophical Transactions for* 1812, *p.* 370.

Periscopic improvement of the camera.

ALTHOUGH the views which I originally had of the advantage to be derived from the periscopic construction of spectacles[*], naturally suggested to me a corresponding improvement in the *camera obscura*, by substituting a meniscus for the double convex lens, I have hitherto deferred making it known to others, except as a subject of occasional conversation.

The mathematical consideration applied to spectacles is not with convenience applicable to the cam. obs.

Since in vision with spectacles, as in common vision, the pencil of rays received by the eye in each direction is small, the superiority of that form of glass, which disposes all parts of it most nearly at right angles with the visual ray, admits of distinct demonstration; but with respect to the camera obscura, where the portion of lens requisite for sufficient illumination, is of considerable magnitude, although it is evident that some improvement may be made in the distinctness of oblique images on the same principles, yet as the focus of oblique rays is far from being a definite point, the degree in which it may be improved is not a fit subject of mathematical investigation.

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[*] Phil. Magaz. Vol. XVII. Nicholson's Journal, VII. 143.

Experiment preferable.

I have therefore had recourse to experiments, in order to determine by what construction the field of distinct representation may be most extended; and I trust the result will be acceptable to this society. I shall take the same opportunity to describe an improvement in the construction of the simple microscope, which may also be termed periscopic, as the object of it is to gain an extension of the field of view, upon the same principles as in the preceding instances, namely, by occasioning all pencils to pass as nearly as may be at right angles to the surfaces of the lens. The mode, however, in which this is effected is apparently somewhat different in the practical execution.

Periscopic microscope.

In the common cam. obs. the side images are indistinct,

In the common *camera obscura*, where the images of distant objects are formed on a plane surface to which the lens is parallel, if the surfaces of the lens be both convex, and equally curved (as in fig. 1, Pl. I); and if the distance of the lens be such, that the images formed in the direction of its axis CF be most distinct, then the images of lateral objects are indistinct in a greater or less degree, accordingly as they are more or less remote from the axis. The causes of this indistinctness may be considered as twofold; for in the first place, all parts of the plane, excepting the central point, are at a greater distance from the centre of the lens, than its principal focus; and secondly, the point *f*, to which any pencil of parallel rays, passing obliquely through the lens, are made to converge, is less distant than the principal focus. On this account, it is in general best to place the lens at a distance somewhat less than that which would give most distinctness to the central images, because in that case a certain moderate extension is given to the field of view from an adjustment better adapted to lateral objects, without materially impairing the brightness of those in the centre. The want of distinctness, however, is even then only diminished in degree, but is not remedied.

because the plane is more distant than the principal focus, and the oblique pencils have a focus still shorter.

New construction. With a meniscus lens concave towards the object, and an aperture at a distance from the concave face.

The construction, by which I propose to obviate this defect, is represented in the second figure, in which are seen the essential parts of a periscopic camera in their due proportion to each other. The lens is a meniscus, with the curvatures of its surfaces about in the proportion of two to one, so placed that its concavity is presented to the objects, and its convexity toward the plane on which the images are formed. The aperture of the lens is four inches, its focus about twenty-two. There is also a circular opening, two inches in diameter, placed at about one eighth of the focal length of the lens from its concave side, as the means of determining the quantity and direction of rays that are to be transmitted.

Statement of its advantages.

The advantage of this construction over the common camera obscura is such, that no one who makes the comparison, can doubt of its superiority; but the causes of this may require some explanation. It has been already observed, that by the common lens, any oblique pencil of rays is brought to a focus at a distance less than that of the principal focus. But in the construction above described, the focal distance of oblique pencils is not merely as great, but is greater than that of a direct pencil. For since the effect of the first surface is to occasion divergence of parallel rays, and thereby to elongate the focus ultimately produced by the second surface, and since the degree of that divergence is increased by obliquity of incidence, the focal length resulting from the combined action of both surfaces will be greater than in the centre, if the incidence on the second surface be not so oblique as to increase the convergence. On this account, the opening E is placed so much nearer to the lens than the centre of its second surface, that oblique rays E*f*, after being refracted at the first surface, are transmitted through the lens nearly in the direction of its shorter radius; and hence are made to converge to a point so distant, that the image (at *f*) falls very nearly in the same plane with that of an object centrally placed.

The oblique pencils have a longer focus than the principal focus.

The aperture in this construction represents by inversion the pupil of the eye in the p. spectacles.

In the use of spectacles by long-sighted persons, the course of the rays in the opposite direction is so precisely similar, that the same figure might serve to illustrate the advantages of the periscopic construction. For the purpose of seeing the extended page of a book (as at AB) with least fatigue to the eye, that form of lens will be most beneficial, which renders the rays received from each part of its surface parallel; and this is effected by the exact counterpart to the preceding arrangement; for in this case the opening E represents the place of the eye receiving parallel rays from the lens in each direction, instead of transmitting them from a distance towards it.

limit of ad-

There is, however, this difference between the two cases, that vantage in the camera.

that in the camera obscura, a much larger portion of the lens is required to conspire in giving a distinct image of any one object; so that the conformation best adapted for lateral objects would not be consistent with distinctness at the centre; and hence arises a limit to the application of the principle. On the common construction, the whole lens is so formed, as to give brilliancy and distinctness at the centre alone, without regard to lateral objects. In adopting such a deviation from the customary form, as I propose, in favour of a more extended view, some diminution of the aperture is required in order to preserve the desired distinctness at the centre. In my endeavours to ascertain the most eligible form of meniscus for this purpose, I have assumed sixty degrees to be the field of view required. But when so large a field is not wanted, then a lens that is less curved will be preferable; and the proportion of the radii must be varied according to the angular extent intended to be included.

Best construction for a large field of view.

Diagram explained by which the radii of lenses are settled.

For the purpose of estimating by what combination of radii any required focal length may be given to a meniscus, I have contrived a diagram by which very much labour of computation may be saved, as a very near result may be obtained by mere inspection. This contrivance is founded on the well known formula for the focal length of any lens $F = \frac{mrR}{R \pm r}$: m being a certain multiple obtained by dividing the sine of refraction by the difference of the sines of incidence and refraction. Hence, in applying this formula to the meniscus, $F : R :: mr : R - r$. In fig. 3, lines expressive of these quantities are so arranged, that by assuming any point F corresponding to the focal length desired, and drawing a line FR through a point R indicating any supposed length of the greater radius, the corresponding length of the other radius will be found where the line drawn intersects the middle line in the diagram.

In laying down these lines, the length and position of AF and AR were assumed at pleasure; and they were divided into any number of equal parts. But the position and length of the middle line Ax was adapted with care to the refractive power of plate glass in the following manner. Since $m = \frac{1}{1{,}505 - 1} = 1{,}98$, a line BC was drawn from the point 10 in the line AR, parallel to AF, and equal to 19,8 divisions of the primary lines; so that if r be $= 10$. then the line BC $= mr$. The distance AC being then divided into ten equal parts, with their subdivisions, afforded the means of continuing the same scale to any desired length. Since the first line BC was laid down parallel to AF, and equal to mr, any other lines drawn through corresponding numbers 7 and 7, 8 and 8, &c. will be also parallel, and by preserving due proportion, will correctly represent mr. Hence in all positions of the line FR, the same similarity of triangles obtains, and the same proportion of $F : R :: mr : R-r$; and consequently the focal length, corresponding to any assumed radii, is truly ascertained.

For the purpose of duly proportioning the curvatures of flint glass, a second line Ay might be laid down in a mode similar to the preceding, by adapting the multiple $m = \frac{1}{1,58-1} = \frac{9}{18}$ to the different density of this glass.

Periscopic simple microscope.

With respect to the construction of a microscope on periscopic principles, I believe the contrivance to be equally new with the former, and equally advantageous. The great desideratum in employing high magnifiers is sufficiency of light; and it is accordingly expedient to make the aperture of the little lens, as large as is consistent with distinct vision. But if the object to be viewed, is of such magnitude as to appear under an angle of several degrees on each side of the centre, the requisite distinctness cannot be given to the whole surface by a common lens, in consequence of the confusion occasioned by oblique incidence of the lateral rays, excepting by means of a very small aperture, and proportionable diminution of light.

Two plano-convex lenses placed face to face with a central aperture in a plate between them.

In order to remedy this inconvenience, I conceived that the perforated metal, which limits the aperture of the lens, might be placed with advantage in its centre; and accordingly I procured two plano-convex lenses ground to the same radius, and applying their plane surfaces on opposite sides of the same aperture in a thin piece of metal (as is represented by a section, fig. 4), I produced the desired effect; having virtually a double convex lens so contrived, that the passage of oblique pencils was at right angles with its surfaces, as well as the central pencil. With a lens so constructed, the perforation that

Dirthat appeared to give the most perfect distinctness was about one-fifth part of the focal length in diameter; and when such an aperture is well centered, the visible field is at least as much as twenty degrees in diameter. It is true, that a portion of light is lost by doubling the number of surfaces; but this is more than compensated by the greater aperture, which, under these circumstances, is compatible with distinct vision.

Application of periscopic principles to the camera lucida.

Beside the foregoing instances of the adaptation of periscopic principles, I should not omit to notice their application to the camera lucida; as there is one variety in its form, that was not noticed in the description which I originally gave of that instrument*.

In drawing, by means of the camera lucida, distant objects are seen by rays twice reflected (*d*, fig 5), at the same time and in the same direction that rays (*e*) are received from the paper and pencil by the naked eye. The two reflections are effected in the interior of a four-sided glass prism, at two posterior surfaces inclined to each other at an angle of 135 degrees. In the construction formerly described, the two other surfaces of the prism are both plane, through which the rays are simply transmitted at their entrance and exit. But since an eye that is adjusted for seeing the paper and pencil, which are at a short distance, cannot see more distant objects distinctly without the use of a concave glass, it may be assisted in that respect by a due degree of concavity given to either, or to both the transmitting surfaces of the prism. It is, however, to the upper surface alone that this concavity is given; for since the eye is then situated on the side toward the centre of curvature, it receives all the benefit that is proposed from the periscopic principles.

VI.

Practical Experiments on hardening Steel. By Mr. E. LYDIATT, *Lecturer on metallurgy, and the mechanic Arts, &c. In a letter from the Author.*

To W. Nicholson, Esq.

SIR,

Introduction.

THE desire I feel to be instrumental in promoting the cause of science and truth, makes me regret that indispensable avocations prevent me from communicating much information to your valuable journal, that would stand a chance, at least, of being useful to many of your readers.

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* Nicholson's Journal, XVII, p. 1. Phil. Magaz. XXVII, p. 343.

Common Tenacity of metals deferred.

To this circumstance alone, is to be referred the delay of my promised communication on the tenacity of the different metals. The time necessarily required to complete experiments on this subject, I have not yet been able to appropriate to that purpose; and I am sorry that it must consequently still stand over, subject however to a determination to fulfil my promise on the earliest opportunity.

In the mean time, a few remarks on interesting mechanical subjects, may not prove unacceptable.

Hardening of steel without warping.

The present paper contains some practical experiments on hardening steel; the results of which have, in a great measure, proved successful in preventing *warping*: an inconvenience hitherto inseparable from the operation.

The usual process bends and spoils the work.

The process usually practised for hardening, is to heat the steel gradually to a red heat, and then plunge it into cold water, which produces the desired effect; but it is a subject of regret with all workers of this metal, that the figure of their work, is frequently changed by the operation, to such a degree, as to render useless all previous labour, and accuracy of workmanship.

The subject does not require theoretical disquisitions.

The limited extent of human knowledge respecting the organization of matter, will only allow us to speak hypothetically as to the occult causes to which these effects are referable. I shall not, therefore, on the present occasion, cloud the investigation of familiar operations, with the subtilties of philosophical disquisition; but proceed to the more useful part of my task.

Heated steel contracts, by cooling, to its first dimensions.

Pyrometrical experiments prove that steel, when heated so as to carry expansion to its utmost limit, if suffered to cool gradually and of its own accord, will return precisely to its original figure and dimensions. The detrimental effects produced by the operation of hardening, must therefore be occasioned, by some derangement of the particles, on the sudden expulsion of caloric. Keeping this idea in my mind, I thought, perhaps, if a piece of steel were repeatedly heated to different degrees below the hardening point, and as frequently quenched in cold water, this process might operate alteratively; and induce a different arrange-

Inference.

arrangement, more favourable to the instantaneous expulsion, of a larger proportion of caloric.

To prove this I made experiments with three cylindrical pieces of steel, six inches long, and half an inch diameter, accurately turned: the first of which I hardened in the usual way, and on examination, found it had deviated from a straight line ·05 In. The second piece, I heated just sufficient to occasion a faint hissing noise when dipped in the water; then a second time a little hotter, and quenched it as before: repeating this operation four or five times, increasing the degree of heat each time; keeping, however, below the hardening point till the last, where it was heated to a blood red and hardened; and to my surprise remained as perfectly straight and unaltered, as before the operation.

Experiments. A piece of steel fully heated and suddenly cooled, warped: another piece repeatedly heated and cooled at successively increased heats did not.

The third piece I treated in the same way, and experienced nearly similar results; and since the time of making these experiments, I have had various opportunities of practising the process; and in every instance have found it effectual beyond my expectation.

A third piece was treated like the second, and with the same result.

For smaller articles, to which the above method is not applicable, I have found that by using water whose temperature is raised to 200°, the steel is not only perfectly hardened, but preserved from the disagreeable consequence, which the use of water at its common temperature, in general produces.

Small articles were hardened in hot water.

In the hope that the results of these experiments may prove useful, I offer them for publicity through the medium of your valuable journal; from which I readily acknowledge to have derived many hints myself, which have proved important in practice as well as theory.

E. LYDIATT.

London, Dec. 5th, 1812.

Annotation. W. N.

As the hardening of steel is higher, and the contraction by cooling, less, the greater the heat of the ignition. I many years ago, endeavoured to equalize the heat by igniting in a bath of red hot lead, (See Philos. Journal, quarto, No. 129.) which I have constantly found to answer. This method is particularly applicable

Another method by heating in melted lead.

cable to broad flat articles or such as have thick and thin parts. Perhaps the combination of both methods may in various cases be found useful.

VII.

Chemical Observations on the Sepia of the Cuttle Fish. By Mr. GROVER KEMP. *Received from the Author.*

Introduction.

THE sepia of the cuttle fish having seldom attracted the notice of chemical writers when treating on animal substances, and its nature and properties being consequently but imperfectly known, the following experiments, it is presumed, will not be unacceptable to the public; since, without being conclusive, they may throw some light on the subject, and open the way to further investigation.

Description of the cuttle fish.

The cuttle fish, called by Icthyologists the sepia, or ink fish, is a genus of vermes mollusca. Its body is often nine inches in length, and three and a half in breadth; the head being attached to it somewhat in the same way as in the tortoise. It has ten tentacula, two of which are longer than the rest, and pedunculated. Its mouth is furnished with a strong beak of a horn colour, the upper mandible of which is hooked like the bill of birds of the falcon tribe. Its back is formed by a peculiar white pithy substance of a friable texture and oblong shape.

Os Sepiæ, or cuttle fish bone.

Singular property of emitting a black liquor,

by which the fish is said to avoid its enemies.

This is the well known Os Sepiæ, or cuttle fish bone of commerce, which is used for taking off the impressions of seals and medals; forming also a common ingredient in dentifrice. It is exactly similar in composition, according to Hatchett, to mother-of-pearl shells, 100 parts consisting of about 24 parts membrane, and 66 carbonate of lime. This bone has no flesh on it, but is merely covered by the external membrane or skin of the fish. A very singular property of this fish is the power which it possesses of emitting voluntarily a black liquor, not out of its mouth, as some naturalists assert, but from a small opening at the upper part of the belly, which communicates by a narrow duct, to a bag or bladder, situated near the coecum, in which this liquor is formed. The cuttle fish is said to avail itself of this property when chased by other fishes, and thus, by rendering the water turbid and opake, it is enabled to elude their search.—Now it is to an examination of this peculiar liquor or sepia, supposed by Rondelet to be the bile of the cuttle fish, that I beg leave to call the attention of the reader; premising, however, that the sepia which I used was taken immediately from the fish, as no dependence can be placed on that which is exposed for sale, which is probably mixed with gum arabic, or some other foreign ingredient.

The sepia, when fresh, is a black glary liquid of a viscid consistence, a peculiar fishy smell and very little taste.

Properties of sepia.

Being subjected to experiment it afforded the following results.

1. It mixed readily with distilled water in any proportion, and shewed little or no disposition to subside after standing many hours: when the mixture was submitted to filtration, a considerable quantity of sepia was left behind, and what passed the filter was a thin black liquid, being a saturated solution of sepia in water.

It mixes with water.

2. Being poured into alcohol it coagulated immediately.

3. The same effect was produced by mixing it with ether.

Coagulates with alcohol and with ether.

4. Alcalies appeared to facilitate the solution of sepia in aqueous menstruum; Potash changing its colour to a brown, but ammonia not affecting it, after, however, it had undergone spontaneous evaporation to dryness, it became sparingly soluble in solutions of pure fixed and volatile alcali, but its colour remained unaltered by either.

Alcalies assist its solution.

5. When some of the saturated solution of sepia was boiled the sepia coagulated.

6. But when a very weak solution of it was boiled, coagulation did not take place.

It coagulates by boiling if saturated, but not if weak.

7. The sepia which was precipitated from its solution by boiling, was soluble in nitric acid when assisted by heat.

The last coagulation was sol. in hot nit. a.

8. After separating by filtration the sepia coagulated by boiling, from the water in which it had been dissolved, a precipitate was obtained by dropping in tinct. of galls.

The clear liquor of No. 5, was precip. by galls.

9. A light brown precipitate was also obtained by adding a solution of oxymuriate of mercury to another quantity of the water.

and also by ox. mur. acid.

10. The sulphuric, nitric, and muriatic acid precipitated the sepia from its solution in water. The sulphuric and muriatic

Sol of sepia is precip. by acids,

did not affect its colour, but the nitric after standing a day or two changed it to a brown.

but not by ox. m. acid.

11. Oxymuriatic acid did not occasion a precipitate with the solution of sepia; and mixed in the proportion of one part of the former to three of the latter, the colour was not affected; but when mixed in equal parts, it was changed to a brown.

Dried s. is insol. in ox. m. a.

12. Sepia, after having been dryed by spontaneous evaporation, was insoluble in oxymuriatic acid.

Ox. mur. of mercury precip. copiously, as does nitrate of silver,

13. A solution of oxymuriate of mercury being added to a solution of sepia, occasioned a copious precipitate.

14. Nitrate of silver precipitated sepia from its solution in water, but did not injure its colour.

and also sulph. of iron.

15. Some solution of sulphate of iron, being dropped into a solution of sepia, the sepia was precipitated, but its colour was not affected.

Deductions.

From the above experiments, particularly from 2, 3, 5, 6, 7, 11, and 13, we may reasonably infer, that the sepia is composed for the most part of albumen. Example 8 and 9 indicate the presence of gelatine.

Sepia stands well as a colour.

As the oxymuriatic and nitric acids have so little effect on the colour of sepia, we may confidently conclude that it possesses the valuable property of standing well. This conclusion is also strengthened, and in a great measure confirmed, by the information of Dr. Leigh, from whom we learn that sepia has been sometimes used as writing ink, and that in a piece of writing of ten years standing, which he had seen, the colour of the sepia was still retained.

Indian ink does not appear to be sepia.

It has been conjectured by some writers,* that Indian ink is nothing else than the sepia of the cuttle fish. A very intelligent gentleman, with whom I corresponded on the subject, and who was of a contrary opinion, writes me as follows: "I have great reason to believe that not a particle of sepia enters into the composition of Indian ink. The colour is very different; and sepia is as superior to Indian ink with respect to the ease of working

Sepia is far superior.

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* "Sepia piscis est qui habet succum nigerrimum instar atramenti quem chinenses cum brodio oriza vel alterius leguminis inspissant et formant, et in universum orbem transmittunt, sub nomine atramenti Chinensis."—Pauli Hermani cynosura, t. 1. p. 17, pars II. Vide etiam Elemens de Chimie, par M. Chaptal, tom. iii. p. 357, Montpellier edit. 1790.

working, as Indian ink is to lamp black. I do not mean to say that it makes a clearer shadow; but Indian ink dries much quicker than sepia—an important consideration where a very large pale shadow is wanted. If too, a mistake be made with sepia, it may be washed almost clean off, whereas part of the Indian ink, if once dry, will adhere to the paper and resist every effort to remove it, without absolutely rubbing up the surface. I could point out other differences between Indian ink and sepia."

To such artists as, by residing near the coast, have an opportunity of procuring the cuttle fish from fishermen, I would recommend the following simple means of preserving the sepia.—After carefully taking the bag out of the fish, having previously secured the duct by a ligature to prevent the sepia from running out, empty the contents of it into a saucer or gallipot, and after spreading it round the sides of the vessel, suffer it to dry gradually by exposure to the air. The reason for only coating the sides of the vessel is in order that it may dry before putrefaction commences.

Sepia dried gradually by exposure to the air may be kept for any time.

In this dry state it will keep for any length of time, and will always be fit for use, by being rubbed up with a little water.

GROVER KEMP.

Brighton, 11 *Mo*. 26, 1812.

VIII.

On the Motions of the Tendrils of Plants. By THOMAS ANDREW KNIGHT, *Esq. F. R. S. From the Philosophical Transactions for* 1812.

THE motions of the tendrils of plants, and the efforts they apparently make to approach and attach themselves to contiguous objects, have been supposed by many naturalists to originate in some degrees of sensation and perception: and though other naturalists have rejected this hypothesis, few, or no experiments have been made by them to ascertain with what propriety the various motions of tendrils, of different kinds, can be attributed to peculiarity of organization, and the operation of external causes. I was consequently induced, during the last summer, to employ a considerable portion of time

The tendrils of plants have been supposed to move from sensation and perception.

time to watch the motions of the tendrils of different species of plants; and I have now the pleasure to address to you an account of the observations I was enabled to make.

Experiments with the Virginia creeper, the ivy, the common vine, and the pea. The V. creeper insulated, turned its tendril towards a wall eight feet distant.

The plants selected were the Virginia creeper (the ampelopsis quinquefolia of Michaux,) the ivy, and the common vine and pea.

A plant of the ampelopsis, which grew in a garden pot, was removed to a forcing house in the end of May, and a single shoot from it was made to grow perpendicularly upwards, by being supported in that position by a very slender bar of wood, to which it was bound. The plant was placed in the middle of the house, and was fully exposed to the sun; and every object around it was removed far beyond the reach of its tendrils. Thus circumstanced, its tendrils, as soon as they were nearly full grown, all pointed towards the north, or back wall, which was distant about eight feet: but not meeting with any thing in that direction to which they could attach themselves, they declined gradually towards the ground, and ultimately attached themselves to the stem beneath, and the slender bar of wood.

Another plant differently situated, turned its tendril to the part most shaded,

and when fully illuminated, turned to an opaque object;

but not to a transparent one,

A plant of the same species was placed at the east end of the house, near the glass, and was, in some measure, screened from the perpendicular light; when its tendrils pointed towards the west, or centre of the house, as those under the preceding circumstances had pointed towards the north and back wall. This plant was removed to the west end of the house, and exposed to the evening sun, being skreened, as in the preceding case, from the perpendicular light; and its tendrils, within a few hours, changed their direction, and again pointed to the centre of the house, which was partially covered with vines. This plant was then removed to the centre of the house, and fully exposed to the perpendicular light, and to the sun; and a piece of dark-coloured paper was placed upon one side of it just within the reach of its tendrils; and to this substance they soon appeared to be strongly attracted. The paper was then placed upon the opposite side, under similar circumstances, and there it was soon followed by the tendrils. It was then removed, and a piece of plate glass was substituted; but to this substance the tendrils did not indicate any disposition to approach. The position of the glass was then changed, and care was taken to adjust its surface to the varying position of the sun, so that the light reflected might continue to strike the tendrils; which then receded from the glass, and appeared to be strongly repulsed by it.

and receded from a radiating one.

The tendrils of the ampelopsis very closely resemble those of the vine, in their internal organization, and in originating from the alburnous substance of the plant; and in being, under certain circumstances, convertible into fruit stalks. The claws, or claspers of the ivy, to experiments upon which I shall now proceed, appear to be cortical protrusions only; but to be capable, I have reason to believe, of becoming perfect roots, under favourable circumstances. Experiments, in every respect very nearly similar to the preceding, were made upon this plant; but I found it necessary to place the different substances, to which I proposed that the claws should attempt to attach themselves, almost in contact with the stems of the plants. I observed, that the claws of this plant evaded the light, just as the tendrils of the ampelopsis had done; and that they sprang only from such parts of the stems as were fully or partially shaded.

The claws of the vine were similarly affected, but at less distances.

A seedling plant of the peach tree, and one of the ampelopsis and ivy, were placed nearly in the centre of the house, and under similar circumstances; except that supports, formed of very slender bars of wood, about four inches high, were applied to the ampelopsis and ivy. The peach tree continued to grow nearly perpendicularly, with a slight inclination towards the front and south side of the house, whilst the stems of the ampelopsis and ivy, as soon as they exceeded the height of their supports, inclined many points from the perpendicular line, in the opposite direction.

The stems of the V. creeper, and the ivy, inclined towards a tree.

It appears, therefore, that not only the tendrils and claws of these creeping dependent plants, but that their stems also, are made to recede from light, and to press against the opaque bodies, which nature intended to support and protect them.

Not only the tendrils, but the stems of plants, incline to their supports.

M. Decandole, I believe, first observed, that the succulent shoots of trees and herbaceous plants, which do not depend upon others for support, are bent towards the point from which they receive light, by the contraction of the cellular substance of their bark, upon that side, and I believe his opinion to be perfectly well founded. The operation of light upon

This effect is opposite to the inclination of succulent plants.

upon the tendrils and stems of the ampelopsis and ivy, appears to produce diametrically opposite effects, and to occasion an extension of the cellular bark, wherever that is exposed to its influence; and this circumstance affords, I think, a satisfactory explanation why these plants appear to seek and approach contiguous opaque objects, just as they would do, if they were conscious of their own feebleness, and of power in the objects to which they approach, to afford them support and protection.

The vine confirms the same explanation.

The tendril of the vine, as I have already stated, is internally similar to that of the ampelopsis, though its external form, and mode of attaching itself, by twining round any slender body, are very different. Some young plants of this species, which had been raised in pots in the preceding year, and had been headed down to a single bud, were placed in a forcing-house, with the plants I have already mentioned; and the shoots from these were bound to slender bars of wood, and trained perpendicularly upwards. Their tendrils, like those of the ampelopsis, when first emitted, pointed upwards; but they gradually formed an increasing angle with the stems, and ultimately pointed perpendicularly downwards; no object having presented itself to which they could attach themselves.

Other plants of the vine.

Other plants of the vine, under similar circumstances, were trained horizontally; when their tendrils gradually descended beneath their stems, with which they ultimately stood very nearly at right angles.

A third set of plants were trained almost perpendicularly downwards; but with an inclination of a few degrees towards the north; and the tendrils of these permanently retained very nearly their first position, relatively to their stems; whence it appears, that these organs, like the tendrils of the ampelopsis, and the claws of the ivy, are to a great extent under the control of light.

The vine differs from the creeper.

A few other plants of the same species were trained in each of the preceding methods; but proper objects were placed, in different situations, near them, with which their tendrils might come into contact; and I was by these means afforded an opportunity of observing, with accuracy, the difference between the motions of these and those of the ampelopsis, under similar circumstances. The latter almost immediately receded from light, by whatever means that was made to operate upon them;

and they did not subsequently shew any disposition to approach the points from which they once receded. The tendrils of the vine, on the contrary, varied their positions in every period of the day, and after returned again during the night, to the situations they had occupied in the preceding morning; and they did not so immediately, or so regularly, bend towards the shade of contiguous objects. But as the tendrils of this plant, like those of the ampelopsis, spring alternately from each side of the stem, and as one point only in three is without a tendril, and as each tendril separates into two divisions, they do not often fail to come into contact with any object within their reach; and the effects of contact upon the tendril are almost immediately visible. It is made to bend towards the body it touches, and if that body be slender, to attach itself firmly, by twining round it, in obedience to causes which I shall endeavour to point out.

Explanation of the causes why the tendril of the vine assumes its motions and curvature.

The tendril of the vine, in its internal organization, is apparently similar to the young succulent shoot, and leaf-stalk, of the same plant; and it is as abundantly provided with vessels or passages for the sap; and I have proved, that it is alike capable of feeding a succulent shoot, or a leaf, when grafted upon it. It appears, therefore, I conceive, not improbable, that a considerable quantity of the moving fluid of the plant passes through its tendrils; and that there is a close connection between its vascular structure and its motions.

I have proved, in the Philosophical Transactions of 1806, that centrifugal force, by operating upon the elongating plumules of germinating seeds, occasions an increased growth and extension upon the external sides of the young stems, and that gravitation produces correspondent effects; probably by occasioning the presence of a larger portion of the fluid organizable matter of the plant upon the one side, than upon the other. The external pressure of any body upon one side of a tendril, will probably drive this fluid from one side of the tendril, which will consequently contract to the opposite side, which will expand; and the tendril will thence be compelled to bend round a slender bar of wood or metal, just as the stems of germinating seeds are made to bend upwards, and to raise the cotyledons out of the ground; and in support of this conclusion I shall observe, that the sides of the tendrils where in contact with the substance they embraced, were compressed and flattened.

The tendrils of the pea are affected as those of the vine.

The actions of the tendrils of the pea were so perfectly similar to those of the vine, when they came into contact with any body, that I need not trouble you with the observations I made upon that plant. An increased extension of the cellular substance of the bark upon one side of the tendrils, and a correspondent contraction upon the opposite side, occasioned by the operation of light, or the partial pressure of a body in contact, appeared in every case which has come under my observation, the obvious cause of the motions of tendrils; and therefore, in conformity with the conclusions I drew in my last memoir, respecting the growth of roots, I shall venture to infer, that they are the result of pure necessity only, uninfluenced by any degrees of sensation, or intellectual powers.

T. A. KNIGHT.

Downton, April 27, 1812.

IX.

Additional Experiments on the Muriatic and Oxymuriatic Acids. By William Henry, *M. D. F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester. From the Philosophical Transactions*, 1812.

Introductory remarks.

THE experiments, which form the subject of the following pages, are intended as supplementary to a more extensive series, which the Royal Society did me the honour to insert in their Transactions for the year 1800*. Of the general accuracy of those experiments, I have since had no reason to doubt; and their results, indeed, are coincident with those of subsequent writers of the highest authority in chemistry. My attention has been again drawn to the subject by the important controversy which has lately been carried on between Mr. Murray and Mr John Davy, respecting the nature of muriatic and oxymuriatic acids†; and I have been induced, by some hints which the discussion has suggested, not only to

Reference to the controversy between Mr. Murray and Mr. John Davy.

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* Page 188.

† Nicholson's Journal, XXVIII, and XXIX.

repeat the principal experiments described in my memoir, but to institute others, with the advantage of a more perfect apparatus than I then possessed, and of greater experience in the management of these delicate processes.

This repetition of my former labours has discovered to me an instance in which I have failed in drawing the proper conclusion from facts. In two comparative experiments on the electrization of equal quantities of muriatic acid gas, the one of which was dried by muriate of lime, and the other was in its natural state, I found a difference of not more than one *per cent.* in the hydrogen evolved, relatively to the original bulk of the gas*. Yet, notwithstanding these results, I have expressed myself inclined to believe, that some water is abstracted by that deliquescent salt; and this belief was confirmed, several years afterwards, by the event of an experiment in which muriatic acid gas, dried by muriate of lime, gave only $\frac{1}{35}$ its bulk of hydrogen†, a proportion much below the usual average. The question, however, was too interesting to be left in any degree of uncertainty; and I have, therefore, made several fresh experiments with a view to its decision. In the course of these I have found, that though differences in the results are produced by causes apparently trivial, some of which I shall afterwards point out, yet that under equal circumstances, precisely the same relative proportion of hydrogen gas is obtained from muriatic acid gas, whether exposed or not to muriate of lime; and that its greatest amount does not exceed $\frac{1}{16}$ or $\frac{1}{14}$, the original volume of the acid gas.

Uncertainty of an experiment of the quantity of hydrog. evolved from mur. ac. gas dried and not dried.

The quantity is the same, whether the muriatic acid gas be exposed or not to muriate of lime.

In the paper last quoted‡, I have also described an experiment, in which sensible heat was evolved by bringing muriate of lime into contact with muriatic acid gas; a fact which, if established, would go far to prove the existence of water in that gas. But on repeating the experiment with muriate of lime recently cooled from fusion, and over mercury carefully deprived of all moisture by boiling, I was not able to discover any increase of temperature, though a very sensible air thermometer was inclosed in the vessel containing the gas. The evolution of heat takes place, only when the muriate of lime

Muriate of lime does not unless humid, evolve heat with muriatic acid gas.

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* Page 191. † Phil. Trans. 1809, page 433.

‡ Page 433, note.

has attracted moisture, either from the atmosphere or the mercury, and is then owing to a condensation of a part of the gas.

Muriatic acid gas, over mercury when electrified, affords calomel and hydrogen; but to a certain extent only.

Essentially, the changes produced by electrifying muriatic acid over mercury are those which I have stated; viz. a contraction of the volume of the gas, the formation of muriate of mercury (calomel,) and the evolution of hydrogen. Recent experiments, also, have confirmed the accuracy of the observation*, that when a certain effect has been produced by electricity, nothing is gained by continuing the process; for neither is more hydrogen evolved, nor can the contraction of bulk be carried any farther.

Muriatic acid gas, electrified in a vessel, without the presence of any other fluid,

I have lately applied, to experiments on muriatic acid, an apparatus which I used advantageously for the analysis of ammonia†. It consists of a spherical glass vessel, into which are hermetically sealed two small tubes containing platina wires, the points of which approach within the striking distance. To the globular part is attached a neck, which may be closed, as occasion requires, either by a glass stopper, or by a metal cap and stop-cock. Into a vessel of this kind I introduced 4½ cubic inches of muriatic acid gas, and passed through it 3000 discharges from a Leyden jar; at the close of the process, no traces of moisture could be perceived on the inner surface of the vessel; nor could I discover, on opening the stopper, that any change of bulk had taken place. After absorbing the unchanged muriatic acid gas by a small quantity of water, a volume of gas remained, in which there were present 100 measures (each equal to one grain of mercury) of oxymuriatic acid gas, and 140 measures of hydrogen. Two causes might, perhaps, contribute to diminish, in some degree, the proportion of the former. It was difficult to exclude from the apparatus, on admitting the muriatic acid gas into it, two or three very minute globules of mercury, which became tarnished during the experiment, exactly as they would have been by oxymuriatic acid; and a small portion of the latter gas was probably also taken up by the water employed to absorb the muriatic acid.

showed no moisture;

but when the muriatic acid gas was abstracted, the small residue was oxymuriatic acid gas and hydrogen.

Repetition on

With the intention of giving greater effect to the electricity,

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* Phil. Trans. 1800, p. 192.

† Ibid. 1809.

I repeated the experiment in a vessel capable of containing not more than 1400 grains of quicksilver, (about ·41 of a cubic inch,) the neck of which being only one-fifth of an inch in diameter, was better calculated to show any minute change in the volume of the gas. On removing the stopper, however, no change of volume was apparent. The hydrogen evolved, instead of being more than in the former experiment, equalled in bulk only 20 grains of mercury. The production of oxymuriatic acid was sufficiently evinced by its effect in tarnishing some very small globules of quicksilver, which adhered to the inside of the vessel; but the minuteness of the quantity frustrated an attempt to measure it. From subsequent experiments on similar quantities of gas, confined in the same apparatus, it appeared, that the electrization in this last instance, had been continued much longer than was necessary; and that an equal effect was produced by one-eighth the number of electrical discharges.

a smaller scale, with the same result.

In this way of making the experiment, the greatest proportion of hydrogen gas obtainable from muriatic acid, amounted only to about $\frac{1}{70}$th, while, by electrization over quicksilver, $\frac{1}{16}$ or $\frac{1}{14}$ was generally evolved. It was evident, then, that the mercury had considerable influence over the results; and I found, by experiments with tubes of different diameters, that the larger the surface of the mercury exposed to the gas, the more rapid and complete was the change. Its action was greatly accelerated, also, by causing the electric discharge to strike from the conducting wire, sealed into the tube, to the mercury, which was probably thus raised into vapour; for in some instances, the whole of the inner surface of the glass was coated with sublimed calomel.

The hydrogen evolved when mercury is not present, is 1-70th; but if present, 1-15th, or nearly five times as much.

The only way in which the mercury appeared to me likely to be efficient in this case, was by removing the oxymuriatic acid as fast as it was formed; for I have never found any mixture of this gas in the results of experiments on muriatic acid, when carried on over quicksilver. Upon any theory of the constitution of muriatic acid, it may be expected that when, in a mixture of that acid gas with hydrogen and oxymuriatic acid gasses, the two latter come to bear a certain proportion to the former, they will be brought within the sphere of mutual agency, and will reproduce muriatic acid. This point appears,

The mercury appears to act by removing the oxymuriatic acid as formed; and preventing the reproduction of common muriatic acid, when the oxymuriatic acid became abundant;

from

viz. both gases to the muriatic as 1 : 35.

from my experiments, to be attained, when the hydrogen and oxymuriatic acid, taken together, have the proportion to the muriatic acid, of about 1 to 35. The amount of the change, therefore, which is capable of being effected on muriatic acid gas, electrified without the contact of mercury, is limited by the reaction of the evolved hydrogen and oxymuriatic acid gasses on each other, whenever they compose a certain proportion of the mixture. This proportion being attained, we only, by continuing the electrization, work in a circle.

Muriatic acid over mercury,

with about 1-13th its bulk of hydrogen, is not changed by electrization,

It may now be inquired, what is the limitation to the action of electricity on muriatic acid gas, which is confined over mercury? In this case it was suggested to me by Mr. Dalton, who favoured me with his presence at most of the experiments, that the evolved hydrogen might possibly in some way prevent the effect from being carried beyond a certain amount. Availing myself of this hint, I mixed thirty measures of hydrogen gas with 400 of muriatic acid gas in its ordinary state, and passed 900 discharges through the mixture. It soon became evident that the addition of the hydrogen had produced an important difference in the results of the experiments; for the surface of the mercury, over which the gas rested, was untarnished after some hundred explosions, and was scarcely changed at the close of the process. When the residuary gas, the volume of which remained unaltered, was analyzed, it was found to contain the same quantity of muriatic gas, as at the outset, and neither more nor less hydrogen.

because the decomposed water finds hydrogen to recompose it:

or the muriatic acid is decomposed and recomposed.

To explain the event of this modification of the experiment, on the old theory, we may suppose, that, by the action of electricity, a particle of water is decomposed, and that the atom of oxygen, forcibly repelled from that of hydrogen with which it was associated, finds another atom of hydrogen uninfluenced by the electric fluid, and within the sphere of its attraction. With this it unites, and recomposes water. On the theory of Sir H. Davy, the same series of decompositions and recombinations may be assumed to take place between the oxymuriatic acid and hydrogen*.

It

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* I am aware, that there is an apparent inconsistency in supposing changes of precisely an opposite kind to be effected by the same means. But instances are not wanting, in which the very same elements are brought into combination by electric discharges, and are again disunited

by

It still, however, remains to be determined, what is the source of the hydrogen gas, which, in a limited proportion, is always evolved by the electrization of muriatic acid? Does it result from the decomposition of water, existing as an element of the gas; or from the disunion of the oxymuriatic acid and hydrogen, which, according to Sir H. Davy's view, compose muriatic acid? The limitation to its amount, which, it formerly appeared to me*, could only be accounted for by the complete destruction of the water contained in the gas, may now be equally well explained, on the principle which I have just pointed out. The fact, also, that no appreciable change of bulk is produced by the electrization of the muriatic acid, when the presence of mercury is excluded, is perhaps favourable to the new theory. For since equal measures of hydrogen and oxymuriatic acids afford muriatic acid without any condensation of volume, no alteration of bulk should result from the disunion of those elements; and the products should be equal measures of the same gases. The proportions, which I obtained (100 to 140) did not, it must be acknowledged, exactly correspond with the theory; but the difference was not greater, than might naturally be expected from the circumstances of the experiment. That equal measures of hydrogen and oxymuriatic acid are really evolved, appears to me to be proved by the agreement, which I have in several experiments remarked, between the hydrogen gas obtained, and the contraction of volume in muriatic acid electrified over mercury. Now the latter effect of the process can be explained on no other principle than the absorption of oxymuriatic acid by the quicksilver.

Qu: Whence comes the limitedhydrog. evolved by electricity? —from water as an element in the gas? —or from the disunion of hydrog. from oxymur. acid as elements of mur. acid?

Perhaps the latter; because the volumes are equal.

When muriatic acid and oxygen gases are electrified toge-

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by the same agency. As examples, it may be sufficient at present to state, that nitrous acid and nitrous gas are generated by the action of the electric spark on mixtures of oxygen and nitrogen gases; and that, by the same power, they are again resolved into their elements. If this were the proper place, it might, I think, be rendered probable by several arguments, that electricity, when thus applied acts rather by mechanical collision, than by inducing a change in the electrical states of the elements of bodies.

* Phil. Trans. 1800, p. 200.

Muriatic acid and oxygen gases, electrified alone, afford *water* and oxymuriatic acid gas.

ther over mercury, a gradual diminution ensues in their bulk,* and the mercury becomes tarnished, precisely as by the contact of oxymuriatic acid. I have lately examined the agency of this process on a considerable quantity of the two gases confined in a vessel, into which they were admitted after exhausting it by the air-pump. The phenomena, which in this way of making the experiment are extremely decisive and interesting, are the production of water and of oxymuriatic acid. The former, combining with a portion of the undecomposed muriatic acid, is deposited in drops upon the inner surface of the vessel, in the state of liquid muriatic acid. When the stop-cock, which confines the gases, is opened under mercury, a quantity of that metal rushes in, and has its surface instantly tarnished. Besides this test of the production of oxymuriatic acid, its presence is rendered unequivocal (after absorbing the undecomposed muriatic acid by a few drops of water), both by its smell, and by its effect in discharging the colour of litmus paper†.

These results agree with either theory. The oxigen may unite with the muriatic acid and water be *deposited*,—or the oxigen may unite with hydrogen as one of the principles of mur. acid and *form water*, *while the* oxymur. acid is disengaged as the other principle.

These results, it will be found, may be reconciled with either theory. According to the one which has been commonly received, the oxygen unites with the real acid of muriatic gas, which becoming oxymuriatic acid, *deposits* water. On Sir H. Davy's view, the oxygen unites with the hydrogen of the muriatic acid, and *composes* water, while the oxymuriatic acid is merely an educt. I am not aware of any refinement of the process, by which the value of these two explanations can be compared. Something, however, would be gained by a precise determination of the proportions, in which the two gases saturate each other. For since, on Sir H. Davy's theory, muriatic acid contains half its volume of hydrogen gas, two measures of which are known to be saturated by one of oxygen, it follows that muriatic acid gas should be changed into oxymuriatic by one-fourth of its bulk of oxygen. According to Gay Lussac and Thenard[*], three measures of muriatic acid should condense one of oxygen (or only one-third their bulk), and should form two measures of oxymuriatic acid. Hitherto, I have not been able to satisfy myself respecting the true proportions of oxygen and muriatic acid gases, that are capable of being united by electricity; for though I have made several experiments with this view, they have not agreed in yielding similar results. The condensation of a part of the undecomposed acid by the water, which is formed during the process, will, probably, indeed, always be an impediment to our learning these proportions exactly. The fact is chiefly of value, as it affords an example of the production of oxymuriatic acid under the simplest possible circumstances; and as it shews unequivocally that, under such circumstances, the visible appearance of moisture is a part of the phenomena.

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* Phil. Trans. 1800, p. 193.

† Those who wish to repeat this experiment need not be deterred by the apprehension of the labour attending it; for 3 or 400 discharges, from a Leyden jar of moderate size, are sufficient to occasion a distinct precipitation of moisture. When a mixture of oxygen and muriatic acid gases is even suffered to stand over mercury, a gradual contraction of volume takes place; the muriatic acid, if in proper proportion, entirely disappears; and calomel is deposited upon the surface of the glass vessel; but, in this case, there is no visible production of moisture.

It may be of value to theory to ascertain the proportions of oxygen and mur. acid gas which form oxym. acid. but this has not yet been effected.

Manchester, Jan. 6, 1812.

XI.

Experiments on Putrefaction. By John Manners, *M. D. of Philadelphia. In a letter from the Author.*

To Mr. Nicholson.

SIR,

FROM reading a paper upon the *vinous* and *putrefactive* fermentation by Gay Lussac, in a late number of your "Philosophical Journal," in which the author, according to the general opinion of chemical philosophers, contended that the access of atmospheric air or oxygen gas, was a *sine qua non* of the process, I was induced to institute the following experiments on *putrefaction*, by which I have proved (as I conceive) beyond the possibility of exception, that oxygen is not only unessential to the putrefactive fermentation, but has, when in actual contact with the putrefying substance, no influence on that process.

Whether oxygen be requisite to putrefaction.

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[*] Mémoires d'Arcueil, ii. 217.

Muscular flesh was included in air above distilled water.

I secured some fresh muscular flesh (a portion of lamb) in the bottom of a glass jar, and inverted it over distilled water, observing that the water within the jar was precisely on a level with that which was external; that any absorption of either of the components of the intercluded atmospheric air, might be noted by a corresponding absorption of water within the jar.

Fahrenheit's thermometer stood at 70°. At which temperature it was kept during the experiment.

The water contained no oxygen.

That the distilled water was perfectly free from any oxygen gas, I proved by Mr. Dusseu's method: viz. I tinged a portion of the same water with *litmus*, and passed *nitrous gas* through it, which Dr. Priestley proved would combine with the oxygen, and be converted into *nitric acid*, which would change the litmus *red*. Dr. Thompson says, however, that this is not a critical test, and that the litmus will not be charged unless there be an unusual quantity of oxygen gas present.

Remarks on eudiometry.

Upon the discovery of this property of nitrous gas, by Dr. Priestley, he founded the first eudiometer, which has been since improved by Falconer, Fontana, Cavendish, Ladriani, Magellan, Baron Von Humboldt, Engenhausz, Dalton, and Gay Lussac, and contributed so much to extend the bounds of philosophical knowledge. Before this important era, the only eudiometer in the hands of the philosophers, was a *sparrow*, a *mouse*, or a *taper*. Since, however, others have been devised; as the sulphuret of iron by Scheele, the liquid hydro-sulphuret of potash by De Marti, the rapid combustion of phosphorus by Humboldt and Seguin, the slow combustion of phosphorus by Berthollet, the green sulphate and muriate of iron impregnated with nitrous gas by Davy, and the detonation of hydrogen gas by Volta.

The putrefaction made no change in the included air

The jar remained three days, during which time the flesh had undergone the putrefactive process, as was evinced by the offensive odour emitted. But at no time could I observe any absorption of water within the jar: except where there was a corresponding reduction of atmospheric temperature, and in consequence a condensation of the intercluded air. But Dr. Priestley, in a similar experiment, found a small augmentation of air within the jar; as I have in subsequent experiments.

The confined air was analysed with the eudiometer of

Humboldt, but not found to differ from atmospheric air in the proportion f its oxygen and nitrogen.

The state of the *barometer*, however, was not attended to in this experiment, which renders it liable to exception. Neither did Dr. Priestley attend to the state of the barometer in his experiments, or if he did he omitted to mention it.

Repetition of the experiment over mercury; with no absorption.

I repeated this experiment over mercury. The thermometer as before stood at 70, and the barometer at 29·1 inches. The experiment was continued three days, when the putrefactive fermentation had taken place, as was evinced by the odour emitted: but there was at no time any absorption of mercury within the jar. Upon examining the included air with the eudiometer it was not found to differ from atmospheric air.

The same repeated with a different mercurial apparatus,

But to magnify and render more conspicuous any absorption, in consequence of a diminution of the included atmospheric air, by the combination of its oxygen with the animal matter, I invented an instrument which I shall now describe.

I took a cylindrical bottle perfectly transparent, and put half a pound of muscular flesh (a portion of the diaphram of a bullock) in the bottom of it and secured it there. The flesh was taken while warm, and cooled under mercury to prevent the access of air. To the bottle was adapted a cork which was perforated, and a bent tube passed through the perforation, the other end of which was hermetically sealed. Some mercury was then put into the bottle—the bottle corked and made perfectly air-tight by luting and sealing—the bottle was now inverted. The mercury filled about two inches of the neck of the bottle, and was made to pass up the glass tube by heating it, and expanding the air, and thus expelling a portion of it to a proper distance. In this situation the bottle was put to rest in a fixed position. A thermometer was included within the bottle in order to note the temperature.

The bottle and curved tube in some measure represented Mr. Leslie's Differential thermometer. The barometrical influence was perfectly excluded. And as variations in the temperature equally affected both the air included in the tube, and that in the bottle, it is evident that thermometrical influence could not affect the experiment. To the tube was adapted a graduated scale, which would mark any rise or fall of mercury in the tube.

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gasometer.

Putrefaction effected without access of air:

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every circumstance as b
for *carbonic acid* gas:
putrefaction [illegible] as

Repetition of the experiments with flesh closely surrounded or immersed in mercury.

I took an eight ounce phial and put six ounces of fresh beef (a portion of the diaphram) in the bottom of it, and secured it there. In procuring this beef I was so careful as to go to the butcher's myself, and have it cut off the moment the animal was dead. Upon this meat while thus warm, and not affected by external air, I placed a column of mercury, by filling the phial with that fluid. The phial was corked, and to the cork was adapted one leg of a syphon, which perforated the cork, all which was made perfectly air tight by luting and sealing.

The syphon was filled with mercury completely, and passed into the mercurial cistern. Over this was placed a glass vessel filled with mercury and inverted, in order to collect any gas that should come over.

That there was now a complete column of mercury from the meat to the top of the vessel inverted in the cistern. My object in the *first* place, was to prove by the first phial containing the meat covered with a column of mercury, whether putrefaction could take place in that situation where the possible access of air was cut off by the mercury. My object with the syphon and other apparatus was to collect and to examine the products, if putrefaction should proceed: the thermometer stood during this experiment at 70°.

In about three days the putrefactive process was evidently going on.

Conclusion. That oxygen is neither essential to, nor has any influence in, putrefaction:

These experiments were sufficient to satisfy me, that atmospheric air or oxygen gas, is so far from being essential to putrefaction, that it has no influence on that process where it has free access to the putrefying substance. These experiments have since been repeated and confirmed by my friend Dr. Mitchill at my request.

but that it is caused by changes in the substance itself.

Therefore I am disposed to believe, that putrefaction must depend on the destruction of the equilibrium of attractions, which in the living state of animals exists among the *elementary principles* of which they are composed, by the loss of *vitality*: by which new attractions are called into action, and new *combinations* and *decompositions* take place.

Products of putrefaction

My next object was to examine the products of putrefaction which had taken place without *extrinsic* oxygen.

Serum.

The *first* product was a *bloody serum*.

Gas.

The second was a transparent gas, possessing the *transparency*, *elasticity, dilatibility, compressibility,* and other mechanical properties of atmospheric air.

As the gaseous products of putrefaction had never been collected and chemically examined, I thought it an object of importance to give it a *careful* and *critical* analysis.

Examination of the gas from putrefaction;

I therefore proceeded to examine it in the following manner.

As it has been the unanimous opinion of chemical philosophers, who have written upon the subject, that *ammonia* is generated, and is the principal product of putrefaction, I first tested for that substance.

by litmus and acid; no change.

1st. By passing a piece of *litmus paper, reddened by vinegar,* into a vessel about half filled with this gas over mercury—no change.

2d. Some of the gas was passed through an *infusion of litmus* reddened by vinegar—no change.

3d. I filled a vessel with mercury over the mercurial bath, and displaced about half of it by passing up an infusion of *litmus reddened by vinegar.* After which I passed up the gas, which was somewhat *absorbable,* until it was strongly impregnated with it, and had accumulated in the top of the vessel—no change.

by turmeric; no change.

4th. 5th. and 6th. I tested it with *turmeric* in all the three ways in which litmus reddened by vinegar was used—no change.

The gas was not absorbed by water.

7th. I passed up a piece of *wet sponge* by means of a wire, but there was no *perceptible* absorption of the gas by the water contained in the sponge.

8th. The sponge was withdrawn and washed in a solution of sulphate of copper—no change.

by sulphate of copper; no change.

9th. I passed up a solution of sulphate of copper into a vessel filled with mercury over the mercurial cistern until it was two thirds displaced by the solution of copper. I then passed up the gas until the solution was strongly impregnated with it and it had accumulated in the top of the vessel—no change.

by carbonic acid gas; no change.

10th. *Carbonic acid gas* was passed up into a vessel containing this gas. No chemical change (except with Mr. Berthollet we call the admixture of gases chemical *dissolution.*) The carbonic acid produced an augmentation in the bulk of the gases proportional to its quantity.

11th. *Muriatic acid gas* was passed up into a vessel filled with gas over mercury—no change.

nor by muriatic acid gas.

These experiments were abundantly sufficient to prove that there was no *ammonia* in the products of putrefaction, at least where it takes place without the influence of external causes.

Secondly, I tested it for oxygen in the following manner.

1st. A piece of *phosphorus* was passed up into a vessel filled with this gas and standing over mercury. The phosphorus was fused and became perfectly fluid, floating upon the surface of the mercury, by pouring boiling water upon the vessel. But there was not the *slightest appearance of combustion.*

Tried by phosphorus; no combustion.

2d. *Water* was now passed into the same vessel, which was tested *for phosphoric acid by litmus*—no change.

3d. *Nitrous gas* was passed into a vessel filled with this gas over mercury—no change, except in the bulk proportional to the gas added.

by nitrous gas; no diminution.

4th. Water was now passed up into the same vessel and tested for *nitric acid* by litmus—no change.

5th. A mouse was passed into a vessel of this gas which instantly died.

by a mouse.

These experiments were deemed sufficient to prove the *nonexistence* of *oxygen.*

Conclusion; no oxygen present.

Thirdly, It was now tested for *sulphuretted hydrogen.*

1st. A piece of *silver* was placed in a vessel of this gas which was *not blackened* or converted into a sulphuret when withdrawn. A piece of silver was also kept in water highly impregnated with this gas, and one was placed at the end of the tube from which the gas was disengaged, and with the *same result.*

Silver shewed no sulph. hydro.

2d. The gas was passed through a solution of *nitrate* of *silver* —no change.

Nitr. of sil. no change.

3d. A vessel was filled with mercury over the mercurial cistern, and displaced by passing up a *solution* of nitrate of *silver* until the vessel was half filled with it. After which the gas was permitted to pass up, as it was disengaged from the putrefying substance through the mercury, and through the solution of silver, until it was strongly impregnated with it and had accumulated in the top of the vessel—no change.

Acet. of lead : no change.

4th. A solution of acetate of lead in the same manner as experiment 2d—no change.

5th. A solution of acetate of lead was impregnated with this gas in the same manner that nitrate of silver was in experiment 3—no change.

Conclusion. No sulph. hydrog. was formed.

These tests were sufficient to prove that no sulphuretted hydrogen was formed.

Fourthly, I tested it for *carbonic acid*.

By litmus.

1st. 2d. and 3d. By *litmus* in all *three* of the ways in which litmus reddened by vinegar was used for ammonia—reddened.

and by lime water

4th. I filled a vessel with mercury over the mercurial bath, and passed up a quantity of lime water, after which I passed up the putrescent gas.—The *lime* was *precipitated*.

and then an acid.

5th. The precipitate effervesced with the *oxalic, sulphuric, nitric*, and *muriatic acids*.

The gas proved to be carbonic.

Consequently the gas is carbonic acid : Probably holding a fœtid oil (or some of the animal matter) in solution to which it owes its offensive odour.

From the six ounces of animal substance I have already collected 100 cubic inches of this gas.

I am, dear Sir,
Your obedient servant,
JOHN MANNERS.

Philadelphia, Oct. 12th, 1812.

XII.

Of the excellent Qualities of Coffee, and the art of making it in the highest perfection. By BENJAMIN, COUNT OF RUMFORD, *F. R. S. Abridged from his 18th Essay, published in London*, 1812.

Praises of coffee.

THE Count begins his Essay with an eulogium on coffee. He celebrates it as uncommonly agreeable in its taste, salubrious in its effects, and producing exhilaration which lasts many hours, and is not followed by sadness, languor, or debility. The glow of health, the consciousness of increased vigour of mind it affords, and the uniform experience of many able, brilliant, and indefatigable men of the first talents in its favour—are among the topics on which the animated writer dwells in his praises of this most delightful vegetable. He acknowledges his own obligation to its powers, and society will admit that a more cogent instance could scarcely have been adduced in support of his argument.

The goodness of coffee depends greatly on its preparation:

But there is no culinary process so uncertain in its results as that of making coffee. The same materials, in the same proportions, shall produce good or bad coffee according to the management. If the peculiar aromatic flavour of coffee be dissipated and lost, its exhilarating quality is gone, and all that would have made it valuable. To prepare it as it ought to be done, is the object of the Essay before us.

Particularly the roasting.

Great care must be taken not to roast coffee too much. As soon as it has acquired a deep cinnamon colour, it should be taken from the fire and cooled: otherwise much of its aromatic flavour will be dissipated, and its taste will become disagreeably bitter.

This is best performed in a glass vessel.

In some parts of Italy, coffee is roasted in a thin Florence flask, slightly closed by a loose cork, and held over clear burning coals with continual agitation. No vapour issues from the coffee sufficient to prevent the progress of its roasting from being clearly seen. The Count has adopted this process by using a thin globular vessel of glass, with a long neck, which he closes, when charged, with a long cork, having a small slit on one side, to allow the escape of the vapours, and projecting far enough out of the neck to be used as a handle to turn the vessel round, while exposed to the heat of a chafing dish of coals. This vessel is laid horizontally, and is supported by its neck so as to be easily turned round; which may be done without the least danger, however near the coals, provided the glass be thin, and kept constantly turned.

Instructions for roasting coffee.

In order that the coffee may be perfectly good, and very high flavoured, not more than half a pound of the grain should be roasted at once; for when the quantity is greater, it becomes impossible to regulate the heat so as to be quite certain of a good result. The progress of the operation, and the moment most proper to put an end to it may be judged and determined with great certainty; not only by the changes which take place in the colour of the grain; but also by the peculiar fragrance which will first begin to be diffused by it when it is nearly roasted enough.

Coffee is best, if made immediately after roasting.

If coffee in powder be not defended from the air, it soon loses its flavour and becomes of little value; and the liquor is never in such high perfection as when the coffee is made immediately after the grain is roasted. This fact is well known to those who are accustomed to coffee in countries where the use of it is not controlled by the laws; and if a government be seriously disposed to encourage the use of coffee, the Count considers it as indispensable that individuals should be permitted to roast it in their own houses. But as the roasting and grinding of coffee takes up considerable time, the author describes a contrivance of a canister to keep it in, which has a double cover. This vessel is a cylinder of tin, having a sliding piston within, of the same material, formed like the cover of a box, but having several slits in its sides, by which they are sprung outwards and cause it to retain its place in the cylinder with considerable force. The piston, being pressed down upon the coffee retains it and defends it from the air, while the same object is more completely secured by a common well fitted cover at top. It may be here remarked—that this kind of canister has the advantage of confining the article without including any air in the same space, except what may be diffused between the particles; ---but that, with this exception, a well-corked bottle or other fit vessel may answer the same purpose.

The roasted coffee must be carefully preserved.

Preparation of the beverage from coffee.

After giving instructions for roasting the coffee and keeping it for use when ground, the preparation of the liquor constitutes the next subject of inquiry. Why this should be so uncertain can only be explained by reference to the circumstances on which those qualities depend which are most esteemed in coffee.

A peculiar aromatic substance extracted by boiling water,

Boiling hot water extracts from coffee which has been properly roasted and ground, an arômatic substance of an exquisite flavour, together with a considerable quantity of astringent matter of a bitter, but very agreeable taste; but this aromatic substance, which is supposed to be an oil, is extremely volatile; and is so feebly united to the water that it escapes into the air with great facility.

which is volatile and soon flies off.

If a cup of the very best coffee prepared in the highest perfection, and boiling hot, be placed on a table in the middle of a room, and suffered to cool, it will, in cooling, fill the room with its fragrance; but the coffee, after having become cold, will be found to have lost a great deal of its flavour. If it be again heated, its taste and flavour will be still farther impaired; and after it has been heated and cooled two or three times, it will be found to be quite vapid and disgusting.

Upon this the exhilarating quality depends.

The fragrance diffused through the air is a proof, that the coffee has lost some of its most volatile parts; and as that liquor is found to have lost its peculiar flavour, and also its exhilarating quality, it is inferred, that both these qualities must undoubtedly depend on the preservation of those volatile parts which so readily escape.

It would not escape if the hot liquid had no agitation.

If the liquid were perfectly at rest, the particles which could escape from its surface, would be incomparably less in quantity, than would escape by agitation, which would continually present new portions of the fluid to the air. But all fluids, while heating or cooling, by partial communication, are known to be agitated; a fact long and well known, but particularly explained and insisted upon by our author, in many of his valuable works, and which he again perspicuously and familiarly explains in the present essay. His object is to indicate by what means the heat of the liquor may be uniformly kept up in all its parts: for the consequence being, that the parts will, in those circumstances, be at rest, the motions by which the aromatic parts might have been dissipated, will not take place.

Agitation may be prevented by surrounding the vessel with boiling water or steam.

By pouring boiling water on the coffee, and surrounding the containing vessel with boiling water, or with the steam of boiling water, the coffee itself will be kept permanently at the same heat, and will not circulate, or be agitated.

Coffee requires boiling hot water.

The Count observes, that from the well-known fact, that boiling water is not the most favourable for* extracting the saccharine parts from malt in brewing, he was induced to try a lower temperature than the boiling heat in making coffee; but the coffee did not prove so good. The cold infusion of coffee, which he also tried, was of very inferior quality.

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* I have always understood, that the temperature of boiling is no otherwise exceptionable in brewing, than because it *makes a pudding*; which phrase denotes, that the grains are rendered so adherent to each other, by the sudden and complete extrication of mucilage, that the wort cannot run off—N.

The common method is wasteful and bad.

The common method of boiling coffee in a coffee pot, is neither economical nor judicious. A large quantity of the material is wasted in this method, and more than half of the aromatic parts, so essential to its good qualities, are lost.

One pound of coffee will make 56 cups.

One pound of good Mocha coffee, which, when properly roasted and ground, weighs only fourteen ounces, will make, by proper management, fifty-six full cups of the very best coffee that can be made.

It must be finely ground.

If it be not ground finely, the surfaces of the particles only will be acted upon by the hot water, and the waste will be very great, from the large proportion of coffee left in the grounds.

A coffee cup contains one gill.

The size of a coffee cup in England usually answers to 8⅓ cubic inches, but the Count considers the *gill measure* as a proper standard for *a cup of coffee*, which he therefore adopts. This will fill the former cup to seven-eighths of its capacity, and *a quarter of an ounce* of ground coffee will be fully sufficient to make a gill of the most excellent coffee.

Coffee must be made by percolation, and not by decoction.

The vessels and their dimensions.

It is well known to chemists, that any solvent already in part charged with a substance intended to be taken up, will be less disposed than before to take up any additional quantity*; and upon this is founded the process of percolation or straining, as is practised in brewing and other arts, and has been for some time recommended and used in making coffee. To this the Count gives his approbation. He finds, by experience, that the stratum of ground coffee to be laid upon a perforated metallic bottom of a vessel or strainer, ought to be about two-thirds of an inch thick, and to be reduced by pressure by a piston or flat plate of metal (after levelling it) to less than half an inch. From the data he infers, by a chain of observations, that if the height of a cylindrical vessel or strainer be taken constantly at 5½ inches, the diameter of its bottom must be—To make 1 cup of coffee = 1⅓ inch—2 cups = 2⅛—3 or 4 cups = 2¾—5 or 6 = 3½—7 or 8 = 4—9 or 10 = 4⅜—11 or 12 = 5.

A strainer, a reservoir, and a surrounding boiler.

These strainers are to be suspended in their reservoirs or vessels for containing the coffee, and the whole included in another vessel called the boiler, which is to contain boiling water, kept hot by a lamp, or otherwise. The forms of these are given with drawings, upon which it does not seem needful to enlarge in the present abridgment, because there are several vessels of this description, with the exception of the surrounding boiler, to be found in our shops.

Description of a very simple and cheap apparatus for making coffee.

The reader must have recourse to the essay itself for these and other particulars of considerable interest, and delivered in the familiar and perspicuous style which distinguish the writings of this author. The poor, and those who prefer simplicity of structure to the extremes of perfection, will be gratified by a description of his last apparatus, fig. 8. It is a porcelain, or earthern jug, with a tubular spout, not unlike those which we call milk jugs, except that these commonly have a lip-spout (which would answer nearly as well.) Into the mouth of this is fitted a tin vessel, which fits and descends a little way down. It has a flat bottom perforated with many holes, and a good close cover; and it would be well to have a round plate or rammer, to compress the coffee on its bottom, and defend it from the stream of hot water, when poured in. These several parts are to be dipped in boiling water before using, and the difference between coffee made by this simple and cheap apparatus, of which the mug may also be applied to other uses, and that made by the the most perfect machines, will scarcely be distinguishable.

General considerations or hints respecting the benefits of coffee.

Sufficient length has already been given to our abstract, to forbid us to follow the Count in the explanation of his views directed to the benefit of society, with relation to the comforts of individuals, as well as to the economy of the political aggregate. That it would be preferable to consume an article produced by the colonies of European nations, who demand the manufactures and products of the parent state, instead of sending bullion to China for an article of less value: that it would be preferable that the poor should enjoy the innocent exhilaration of coffee, and the nutriment of sugar, instead of forgetting their hardships during the momentary intervals of insanity, produced by fermented and distilled liquors; that they should be cheerful, benevolent, animated, healthy, and industrious with coffee, instead of becoming outrageous, mischievous, diseased, idle, and sunk in languor and debility with gin, &c. &c. These are among the meditations interspersed through this little work, which the reader will be gratified in consulting, and will probably be induced to make others in his turn.

XV.

Description of an improved Pump for raising the Water from Wells or Mines, while sinking or making. By Mr. William Brunton, *of Butterley Iron Works, in Derbyshire**. *Extracted from the Transactions of the Society of Arts, published in the Year* 1812.

Improved pump for sinking of wells, mine-shafts, &c.

THE contriver of this pump, previous to entering upon a description of his drawings, gives the following statement of the inconveniences he proposes to obviate.

First, as it is necessary for the pumps, whilst sinking, to be always working upon air, that the water may be kept very low in the pit, the engine of course frequently goes too fast, and carries up, by the violence of the current, small pieces of stone, coal, or other substances, and lodges them above the bucket, which must considerably retard the working of the pump, and wear the leather.

Secondly, When the engine is set to work, (after having been stopt whilst working upon air, and consequently a quantity of air remaining in the suction-pipe, with the small stones, &c. deposited on the valves of the bucket) it often happens, that the compression of the air, by the descent of the bucket, is not sufficient to overcome the weight of the bucket valves so loaded with rubbish, and the column of water in the stand pipes, the pump is hereby prevented from catching its water; the usual remedy for which is, to draw the bucket out of the working barrel, until a quantity of water has escaped by its sides, and displaced the air. Observe here, that this often happens from the unnecessary magnitude of the space between the bucket and clack.

Thirdly, The pumps are suspended in the pit by capstan ropes, for the purpose of being readily lowered as the pit is sunk; the stretching of the ropes, (especially when sinking in soft strata,) occasions much trouble, by suffering the pumps to choke; but the most serious evil is, that the sinkers, in shifting the pumps from one place to another, throw them very far out of perpendicular, thereby causing immense friction, and wearing in all the parts; besides endangering the whole apparatus, by breaking the bolts and stays, and straining the joints.

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* For which the silver medal was voted.

Improved pump for sinking of wells, mine-shafts, &c.

Fourthly, As the pumps sink, the delivering pipe at the top is raised, by putting on short pipes, generally about a yard at a time, which occasions many stoppages and much hindrance in the work.

Having an engine pit to sink at Codnor Park Colliery, Derbyshire, belonging to the Butterley Iron Company, I endeavoured to obviate the difficulties stated; and first, for the purpose of preventing the pumps working too much upon air, I constructed a working barrel, (which in this case was nine inches diameter,) with a side pipe three inches diameter, connected therewith by an opening at the top and bottom; also at the upper end of the side pipe I fixed a valve, so as to slide over and shut the communication with the working barrel, the stem of the valve by which it is regulated, passing through a stuffing box, and by letting a quantity of water return through the side pipe to the bottom of the working barrel, (the men at the bottom regulating the valve, so that the pump takes the water as it comes,) very little rubbish is then taken into the pump, and much wear and tear of buckets prevented.

Secondly, I also, by this valve and side pipe, preclude the necessity of ever drawing the bucket to displace the air. The clack piece was made as small as possible, and the clack with its gearing very low, in order to have as little space as possible between the bucket and the clack. The clack, as represented in the drawing, possesses the advantages of being easily caught by the clack hook in case of being under water. The ring prevents it from oversetting, and thereby fastening itself in the pumps, and the valves are very easily repaired by unscrewing the cross-bar, which admits of their being taken off and replaced.

Thirdly, I avoid the inconvenience of suspending the pumps by ropes, by forming the suction-pipe in two pieces, one inner and outer pipe; the outer pipe is bored for about six inches in length, and the inner one turned cylindrically to fit it; they slide into each other the whole length of a regular pipe, viz. nine feet; and they are made tight by collars of leather, surrounded by a cup filled with water and clay. The pumps

Improved pump for sinking of wells, mine-shafts, &c.

pumps are supported at proper distances, so as to suit the length of the pipes, by beams, and across those are other beams, upon which the flanches of the pipes rest: these last are not fastened by any bolt, in order that they may be readily removed. The pumps, by these means, remain stationary, and the suction-pipe lengthens as the pit is sunk, until it is drawn out to its full extent. The whole column is then lowered to the next flanches, and another pipe is added to the top; the lower end of the suction-pipe is formed somewhat like a crank, in order that the sinkers, by turning it round upon the other pipe, may move it from one place to another, and so prevent the necessity of sinking immediately under it.

Fourthly, The pumps being stationary, as above stated, the pipe at the top will of course deliver the water at the same level at all times, and instead of being obliged to lengthen the column every yard sunk, it will only be necessary every nine feet.

Fig. 1. Pl. II, is the section of a shaft or pit, with the pump fixed in it; it is cast in lengths of nine feet each, screwed together by flanches, and supported by beams extending across the pit, (as shown in the plan, fig. 6,): short pieces are laid across these, with half circular holes in them; and these being put round the pump, just beneath a flanch, sustain the pump firmly, but may quickly be removed when it is required to lower the pumps in the pit; and, as they are not fastened, they do not prevent the pumps being drawn upwards; A, fig. 1, is a door which unscrews, to get at the lower valve or clack of the pump; this is more clearly explained in the enlarged section, fig. 2, where A has the same designation, B, fig. 2, is the working barrel, with the bucket D working in it; E is the clack, also shewn enlarged in figs. 3 and 4; F is the suction-pipe, and GG the moveable lengthening piece; this slides over, and includes the other, as in fig. 2, when the pump is first fixed; but, as the pit is sunk, it slides down over the pipe F, to reach the bottom, as in fig. 1; the outside of the inner pipe F is turned true and smooth, and the inside of the outer pipe G, at the upper end, is bored out to fit it; the junction is made perfect by leathers placed in the bottom of the cup *a a*, which holds water and wet clay over them, to keep them wet and pliable, and consequently air-tight; the lower extremity

tmity of the suction-pipe G, terminates in a nose, pierced with a number of small holes, that it may not take up the dirt: this nose is not placed in a line with the pipe, but curved to one side of it, so as to describe a circle when turned round; by this means the sinkers can always place the nose in the deepest part of the pit, as shewn in fig. 1; and when they dig or blast a deeper part, they turn the nose about into it, the sliding tube lengthening down to reach the bottom of it: by this means there is never a necessity to set a shot for blasting so near the pump foot, as to put it in any danger of being injured by the explosion, as is the case in the common pump; in which this danger can only be avoided by moving the pump foot to one side of the pit, which necessarily throws the whole column of pumps out of the perpendicular.

Improved pump for sinking of wells, mine-shafts, &c.

The construction of the clack is explained by figs. 3 and 4, the former being a section, and the latter a plan; LL is a cast-iron ring, fitting into a conical seat in the bottom of the chamber of the pump, as shewn in fig. 2; it has two stems, *l l*, rising from it, to support a second iron ring, M M; just beneath this, a bar, *m*, extends across from one stem to another, and has two screws tapped through it; these press down a second cross-bar, *n*, which presses the leather of the valves down upon the cross-bar of the ring L, and this holds it fast, forming the hinge on which the double valves open, without the necessity of making any holes through the leather, as in common; but the chief advantage is, that, by this means the clack can be repaired, and a new leather put in, in far less time than at present, an object of the greatest importance; for, in many situations, the water gathers so fast in the pit, that if the clack fails, and cannot be quickly repaired, the water rises above the clack-door, so as to prevent any access to it, and there is no remedy, in the common pump, but drawing up the whole pile of pumps, which is a most tedious and expensive operation. In Mr. Brunton's pump, the clack can at any time be drawn out of the pump, by first drawing out the bucket, and letting down an iron prong, fig. 5, which has hooks on the outsides of its two points; this, when dropped down, will fall into the ring M, and its prongs springing out, will catch the underside, and hold it fast enough to draw it up; another part of Mr. Brunton's improvement consists in the addi-

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addition of a pipe H, fig. 2, which is cast at the same time with the barrel, and communicates with it both at the top and at the bottom, just above the clack ; at the upper end the pipe is covered by a flat sliding plate, which can be moved by a small rod, *b*, passing through a collar of leather ; the rod has a communication by a lever, so that the valve can be opened or shut by the men in the bottom of the pit ; the object of this side pipe is, to let down such a proportion of the water, which the pump draws, as will prevent the pump drawing air ; though of course the motion of the engine will be so adapted, as not to require a great proportion of the water to be thus returned through the side pipe ; yet it will not be possible to work the engine so correctly, as not to draw some air, without this contrivance ; and if it does, it draws up much dirt and pieces of stone into the pump, besides causing the engine to work very irregularly, in consequence of partially losing its load every time the air enters the pump. Another use of the side pipe is, to let down water into the chamber of the clack to fill it, when the engine is first set to work, after the pumps have been standing still, and the lower part of the barrel and chamber empty*.

XV.

An Account of an Experiment made in the College Laboratory, Edinburgh, drawn up by JOHN DAVY, *Esq.*

SIR,

If water be not shewn by the combination of mur. a. gas, and amm. gas, *the mur. a. gas contains none.*

IN the preceding numbers of your Journal, several papers have appeared, relative to the result of the combination of muriatic acid and ammonia. Mr. Murray first made the experiment, for the purpose of ascertaining the nature of the former gas—whether it be a compound of an unknown basis and water, or a compound of chlorine and hydrogene. He justly concluded, that if water was obtained from muriatic acid gas, by means of ammonia, its existence in the acid must be admitted; and that, on the contrary, if no water could be procured, it would be unphilosophical to suppose water present; but that muriatic acid gas must be considered as a compound of hydrogene and chlorine. Such were Mr. Murray's premises.

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* This communication was accompanied by a handsome letter from that eminent civil engineer, William Jessop, Esq., who, after explaining the usual practice and the effect of Mr. Brunton's improvement, adds, that simple as it is, it will be found, as he has from experience ascertained, to be of considerable value to those interested in mining concerns.

Asserted fact by Mr. Murray, that the dry gases afford water. Conclusion, *that m. a. gas is water and a basis.*

The result of his experiment, he says, was the production of water from the muriate of ammonia, formed by the union of the dry gases. He therefore, of course, concluded, that muriatic acid gas is not a compound of chlorine and hydrogene, but of water and an unknown basis (muriatic acid;) in fact, that the old doctrines respecting this substance, which he had strenuously defended before, are correct, and the new theory advanced by Sir H. Davy, erroneous.

This experiment was also repeated on a very small scale at Liverpool, by Drs. Bostock and Traill, and with nearly the same result.

Sir H. Davy and the writer obtained no water.

But other results have been obtained.—Sir H. Davy has made the experiment several times, and under different circumstances; and has uniformly perceived no water, when the atmospheric air was excluded, and dry vessels, and dry gases were employed; and my experience is agreeable to his, having been unable to obtain any water the only time I repeated the experiment, on subjecting the muriate to a heat not sufficient for its sublimation, though water was procured by heating the same salt, after it had been exposed to the atmosphere.

It is not my object at present to attempt to reconcile these contradictory results, or to show, by any process of reasoning or criticism, that Mr. Murray has fallen into errour. It is my intention to confine myself to the concise detail of new experiments, which will tend, I trust, to decide the question.

Repetition of the experiment before eminent men.

About two months since, when my brother, Sir H. Davy, was in Edinburgh, he was desirous of repeating the experiment on the combination of muriatic acid and ammonia, with Dr. Hope. The experiment was accordingly made in the College Laboratory.—Sir George Mackenzie, Mr. Playfair, and some other gentlemen, were present.

The amm. gas was dried by

The alkaline and acid gases employed, were pure, and both had previously been dried by exposure, for about sixteen hours, to potash; the mur. a. gas by mur. of lime. They were in alternate portions received in an exhausted vessel. The mur. amm. was driven from the neck of the vessel; and this being cooled, and the body heated,

substances having a strong attraction for water—the ammoniacal gas to pieces of potash—and the muriatic to dry muriate of lime. The apparatus for making the experiment consisted of a plain retort of about the capacity of twenty-six cubic inch measures, with a stop-cock, and of a receiver, with a suitable stop-cock. The latter was filled over mercury with one of the gases, which from the receiver passed into the exhausted retort, by means of the stop-cocks; the other gas was introduced the same way into the retort; and thus alternately about ninety cubic inches of each gas were combined. The muriate of ammonia formed, was of its usual appearance. As it was diffused over the whole surface of the retort, it was necessary to clear the neck by the sublimation of the salt into the bulb, that if any water was present, it might be detected here in the second part of the operation.

All the salt being driven into the bulb of the retort, by the heat of a spirit lamp, the neck was cooled and kept cold by moistened cloths, whilst the bulb was heated by a coke-fire, till the muriate began to sublime, and to make its appearance at the curvature of the vessel. The fire was now withdrawn. It had been gradually and equally applied, and it had been continued for a considerable time.

a dew, just perceptible, was seen in the neck,

The result was examined whilst the bottom of the retort was still very hot, and whilst that part, where a little of the muriate had sublimed, exceeded the temperature of boiling water. A dew just perceptible was observed lining the cold neck. The quantity of water was so extremely small, that the globular particles composing this dew could scarcely be perceived by the naked eye, unassisted by a magnifying glass.

This result appeared to me very decisive. The quantity of gases employed was large; the water, which ninety cubic inches of muriatic acid gas should afford, is, according to hypothesis, equal to no less than eight grains. How great is the difference between this quantity and a dew barely perceptible! which may reasonably be referred to a minute quantity of vapour in the gases, or to a little moisture derived from the mercury, a small quantity of which entered the retort with the gases.

which seemed about one-

Dr. Hope wished to ascertain how much water would produce such a dew as was observed. For this purpose he heated in a retort, of a similar size to that used in the experiment, a single drop of water, which it may be said weighs about 1 grain. The appearance of condensed water in this instance in the neck of the retort, was much greater than in the preceding; he thought that it was 3 or 4 times as great.

sixth of a grain.

May we not conclude from these results, on Mr. Murray's own ground of reasoning, that water is not a constituent part of muriatic acid gas, and that this substance is a compound merely of chlorine and hydrogene? And may we not reasonably consider that very minute portion of water, which did appear, as uncombined moisture derived from various sources? It is easy to account for the presence of about $\frac{1}{6}$ of a grain of water on the one theory; it is impossible to account for the absence of 8 grs. on the other.

Deduction: that water is not a constituent part of mur. a. gas; but that it is a comp. of chlorine and hydrogene.

It has been shewn, by Dr. Henry, that ammonia obstinately retains aqueous vapour; and Sir H. Davy has proved, that a minute portion of solution of muriatic acid in water, may be obtained by intensely cooling the gas. There is great difficulty in drying mercury without boiling it; and in the present instance the mercury was not boiled. These trivial circumstances do not deserve notice, otherwise than as tending to account for the very minute quantity of water obtained. It is probable, judging from the past, that objections will be made, and I wish to anticipate them.

The present mode of heating the muriate of ammonia in a close retort, which had also been adopted on a former occasion, was objected to in a preceding number of your Journal. Mr. Murray there observed, that in consequence of the air being confined, it was possible that the water could not rise in vapour, or at least that it was impeded in its volatilization. His reasoning was subtile, and it would have been plausible had there been no circulation of air in the vessel, and quite correct if the heat employed had not been sufficient to convert the water into an elastic fluid or true gas.—But in a large retort such as we used, there is a circulation of air, when heat is partially applied; and the heat employed was far above that required for boiling water. Not to dwell on reasonings, which on controverted points are always very justly to be suspected, I shall have recourse to fact. A single drop of water was introduced into a retort, about the same size as that employed in the experiment, and it was tightly

If water had been present it would have risen.

Experiment is proof.

tightly stopped by a cork. On the application of heat to the bulb, the water passed off into steam apparently with the same velocity that it would have done, had there been a free communication between it and the atmosphere, and of course the steam was just as readily condensed. This experiment was suggested and made by Dr. Hope.

I have now finished the account of the experiments which I wished to communicate, and as I have no intention of answering personal aspersions, which are only injurious to the author when unjustly made, nor of entering again into a controversy concerning words, I shall here conclude with subscribing myself,

Your obedient humble servant,
JOHN DAVY.

Edinburgh, December 9.

To Mr. Nicholson.

P. S. I have authority from Dr. Hope, and also from Sir George Mackenzie and Mr. Playfair, to mention, that the detail I have given of the experiment is correct.

I should have before observed, that the muriate was heated in the preceding experiment in a partial vacuum. After the combination of the two gases had been formed, a little ammonia remained in the retort, and to this as much air was admitted, as was conceived sufficient when the heat was applied, to produce the common atmospheric pressure.

SCIENTIFIC NEWS.

Account of Books, &c.

Philosophical Transactions of the Royal Society of London, for the year 1812. *Part II. 4to.* 187 *pages, with* 12 *plates.*

THIS part contains the following paper. 1. Observations of a second Comet, with remarks on its construction. By William Herschel, LL. D. F. R. S. 2. Additional Experiments on the Muriatic and Oxymuriatic Acids. By William Henry, M. D. F. R. S. &c. (See our present volume, p. 42.) 3. Of the Attraction of such Solids as are terminated by planes; and l of solids of greatest attraction. By Thomas Knight, Esq. Of the Penetration of an Hemisphere by an indefinite num- r of equal and similar Cylinders. By Thomas Knight, Esq. On the Motions of the Tendrils of Plants. By Thomas ıdrew Knight, Esq. F. R. S. (See our present Vol. p. 37.) Observations on the Measurement of three degrees of the eridian conducted in England by Lieut. Col. William udge. By Don Joseph Rodriguez. 7. An account of some periments on different Combinations of Fluoric Acid. By hn Davy, Esq. 8. On a Periscopic Camera Obscura and icroscope. By William Hyde Wollaston, M. D. Sec. R. S. ee our present vol. p. 26.) 9. Farther Experiments and servations on the Influence of the Brain on the generation of imal heat. By B. C. Brodie, F. R. S. 10. On the different ructures and situations of the solvent Glands, in the digestive gans of Birds, according to the nature of their food, and rticular modes of life. By Everard Home, Esq. F. R. S. . On some Combinations of Phosphorus and Sulphur, and some other subjects of Medical Inquiry. By Sir H. Davy, nt. Sec. R. S. List of Presents. Index.

The History of the Royal Society from its Institution to the ıd of the 18th Century. By Thomas Thomson, M. D. R. S. L. and E. 2 vols. 4to. price 2l. 2s. and on fine paper, . 12s.

Mr. Andrew Horn, of Wycombe, acquaints me that he has a ort *Essay on Vision* in the press, in which the Seat of Vision determined, and by the discovery of a new function in the gan of Sight, a foundation is laid for explaining its mechan- n and the various phenomena, upon principles hitherto un- tempted.

ionomia. Opinions concerning Life and Health, introductory to a Course of Lectures on the Physiology of Sentient Beings. By A. P. Buchan, M. D. of the Royal College of Physicians, London, 8vo. 119 pages, with 8 p. Introduction.

Where the master of a science, not to be acquired without eep erudition, a diligent and correct observation of facts, and ı enlightened spirit of philosophical research, takes his station on an eminence, and by a few striking outlines gives a sketch of the prospects around him, it becomes impossible to make an analysis of his work. I must therefore confine myself to say, that this treatise contains many important and highly interesting truths, delivered with perspicuity and elegance.

M. De Luc's Geological Travels in Germany, France, and Switzerland, are nearly ready for publication.

A work on Oriental Commerce, in two 4to. vols. By Mr. Milburn, with numerous Charts, by Arrowsmith, is expected to be published in a few weeks.

Tyrocinium medicum; or a Dissertation on the Duties of Youth, apprenticed to the Medical profession. By William Chamberlayne, Member of the Royal College of Surgeons, Fellow of the Medical Society of London, &c. duodecimo, 253 pages, London, 1812.

THIS familiar and very perspicuous Dissertation, contains much more than is indicated in the title. It is a subject of primary interest to the public, that the preparation and dispensing of medicines should be done with fidelity, precision, and dispatch. It is of equal importance that the professors of the art should not be deficient in the requisite information. But in every class and every rank of Society, the habits of order, method, cleanliness, punctuality, and other good qualities, which have been called the minor virtues, are so essential to prosperity and happiness, that a book which strikingly displays their advantages, must be considered as of much more extensive utility than any set of Aphorisms confined to an individual profession. The good advice with which this Treatise abounds, is calculated to afford great benefit to the reader, whether intended for the Medical profession, or for any other department of life.

M. Zambeccari, accompanied by a friend, ascended in a balloon from Bologna, on the 21st September. On his descent, the balloon became entangled in the branches of a high tree, and, before it could be disengaged, caught fire. The two aeronauts leaped out. M. Zambeccari was killed upon the spot;

but M. Bologna, his friend, survived, though some of his limbs were broken.

The ascension of Bittorf, the mechanician, from Manheim, was equally disastrous. When he had risen to a considerable height, he perceived, too late, that his balloon was damaged, and he had no other resource than to open the valve. The balloon descended with extreme velocity, and the inflammable matter which it contained, took fire, the shreads of the balloon falling upon M. Bittorf's head and breast, which were much burnt. On a sudden, his crazy vehicle struck upon the roof of a house, two stories high, from which he was precipitated, and died the next day in great agony.

Mr. Sadler, the aeronaut, ascended from Belvidere-house, near Dublin, October 1. at 1 p. m. with the wind at south-west, and in thirty-five minutes had sight of the mountains in Wales; he continued in the same direction till three o'clock, when being nearly over the Isle of Man, the wind blowing fresh, he found himself fast approaching the Welch coast; and at four o'clock, he had a distinct view of the Skerry lighthouse, and the prospect of consummating his ardent hopes of a speedy arrival in Liverpool. The wind now shifting, he was again taken off, and lost sight of land; when, after hovering about for a long time, he discovered five vessels beating down channel; and in hopes of their assistance, he determined on descending with all possible expedition, and precipitated himself into the sea. In this most critical situation he had the mortification to find the vessels took no notice of him: obliged, therefore, to reascend, he now threw out a quantity of ballast, and quickly regained his situation in the air, to look out for more friendly aid. It was a length of time before he had the satisfaction of discovering any; and then observed a vessel, which gave him to understand by signal, that she intended to assist him, but could not reach him. Two others also now appeared in sight, and one of them tacking about, hoisted the Manx colours: night now coming on, he was determined to avail himself of their friendly aid, and once more descended into the sea; but here the wind acting upon the balloon, as it lay on the water, drew the car with so much velocity, that the vessel could not overtake it; and, notwithstanding

standing he used his utmost efforts, and latterly tied his clothes to the grappling-iron, and sunk them to keep him steady; still the balloon was carried away so fast, that he was under the necessity of expelling the gas; upon that escaping, the car actually sunk, and he had now nothing but the netting to cling to. His perilous situation, and the fear of getting entangled, deterred the men from coming near him; until, being in danger of drowning, Mr. Sadler begged they would run their bowsprit through the balloon, and expel the remaining gas. Having done this, they threw out a line, which he wound round his arm, and was then dragged a considerable way before they could get him on board, quite exhausted.

A meteoric stone, of the weight of 15lbs. fell to the earth on the 1st of March, 1811, in the village of Konleghowsk, dependent on the town of Romea, in the government of Tschernigoff, in Russia, and making part of the domains of Count Golovkin. Its fall was preceded by three violent claps of thunder. When it was dug out from the depth of more than three feet, through a thick layer of ice, it still possessed heat. It was remarked, that at the third clap of thunder there was an extraordinary explosion, with a loud noise, and throwing out a great quantity of sparks.

A new comet was discovered by M. Pons, of Marseilles, on the 20th of July. Its course was then between the feet of the Camel-leopard and the head of the Lynx. It was discovered afterwards at Paris, by M. Bouvard; and, according to the calculations of these astronomers, it passed its perihelion on the 15th of September, when its distance from the sun, taking that of the earth at unity, was at 0,77835, and its inclination to the ecliptic is 74° 50′.

The Geological Society held its first meeting of the present session on Friday, November 6th, 1812.

A second letter from Ed. L. Irton, Esq., in answer to some queries by the President, relative to the sand tubes found at Drigg, in Cumberland, was read.

From this it appears, that the tubes have hitherto been found only in a single hill of drift sand on the sea-shore, of the extent of about five acres. The entire form of the tubes is not known; for they are discovered in consequence of being laid bare by the drifting of the sand; and the same cause almost always breaks off, and injures their upper extremity. The manner in which they terminate below, is still less known: one of the tubes was exposed by hazardous digging, in running sand, to the depth of about fifteen feet, without the least appearance of its being about to terminate. They lie parallel to each other, and nearly vertical, but at unequal distances—the number must be very considerable, Mr. Irton having himself taken away, at different times, not less than a hundred.

The tubes, when first dug out, are very flexible, but exposure to the air for a few seconds deprives them of this quality. The unctuosity of the internal glazing of these tubes, when recently dug up, stated by Mr. Irton, in his first letter on the authority of another person, appears, on more accurate examination, to be a mistake.

A communication from George Cumberland, Esq., relative to some lime-stone strata in the neighbourhood of Bristol, was read.

The strata here described compose the rocks opposite to the Hotwell Walks, and are farther illustrated by two drawings; the one of the external face of the rocks, the other of a large cavern recently discovered. In clearing the ground for the erection of houses opposite to the Old York hotel, on Clifton downs, some interesting varieties of sulphate of strontian were met with, but the place being now covered with building and garden grounds, there is little likelihood of its being soon again opened to the researches of the mineralogist.

A communication, accompanied by three drawings in illustration, from Dr. Mac Culloch, Mem. G. S. relative to a remarkable interrupted vein in lime-stone, was read.

This vein occurs in a mill-stone which was shipped from Limerick, and is at present at the royal powder mills at Waltham Abbey. The stone itself is a dark blue slaty limestone, containing comminuted fragments of marine remains; the vein by which it is traversed is whitish compact carbonate of lime. This vein, in its present state, consists of a number of separate angular fragments, having somewhat of a general parallelism with such a correspondence at any two neighbouring extremities as to render it a matter past doubt that they have once formed a continuous vein.

To displace such a vein into its present position, it is necessary to suppose that the rock originally consisted of a series of very thin strata, which, being fissured across, formed a space for the reception of the substance of the vein. It is evident from the angularity and the irregularly-serrated edges of the displaced fragments, that the white calcareous carbonate must have been perfectly indurated at the time of its displacement: yet that the strata of the limestone must have been in a state to admit of a series of shifts or slides, each successively advancing with equal intervals beyond the one preceding it: it is necessary also to suppose that the strata must have been in some condition admitting them to cohere intimately together, either at the period when the slides took place, or afterwards, from the perfect obliteration of the seam. By what theory can these facts be explained?

Friday, Nov. 20.

A communication from Ar. Aikin, Esq. Sec. entitled "Some observations on a bed of Greenstone, near Walsall, Staffordshire, was read.

The Greenstone, which is the subject of this paper, is of a dark blackish-blue green colour; has a glimmering lustre, and an uneven fracture, breaking into irregularly wedge-shaped blunt-edged fragments: it is tough, acquiring a kind of polish under the hammer, is moderately hard, and rather heavy. It strongly attracts the magnetic needle, and effervesces on immersion in cold diluted muriatic acid. It consists principally of felspar, mixed with calcareous spar, with minute shining black grains of Augite, and of hornblende. It is penetrated by nearly vertical contemporaneous slender veins of calcareous spar, and after a few weeks exposure to the air acquires a liver-brown colour and falls to pieces.

It occurs in the independent coal formation; but is not coextensive with this formation; nor indeed in the opinion of the author of the paper is it to be considered as a true bed, but rather a lateral vein branching off from a large dyke of greenstone that comes up to the surface, dividing the colliery in which the greenstone bed is, from another adjacent to it.

On comparing the strata above and below the greenstone, with the very same strata that have been pierced through in a part of the colliery where the greenstone does not occur, it appears, that the bed of slaty clay with balls of ironstone lying upon the greenstone, does not materially differ from the same bed where the greenstone is absent, but that the beds immediately *below* the greenstone, present very different characters where they are covered by this latter from what they do where the contrary is the case. These beds are 1. Sandstone, 2 Bituminous shale, with slender seams of coal, and 3. a coal somewhat more than a yard thick. Of these the sandstone is considerably indurated, the bituminous shale is also indurated, entirely deprived of bitumen, and is broken more or less into irregular pieces, and mixed with the lower part of the sandstone bed. The yard coal is also entirely deprived of bitumen, is stained and irridescent on the surface of its natural joints, and is more friable. These changes appear to accompany the superposition of the greenstone bed through its whole extent, and from the circumstance of their ceasing where the greenstone terminates, they appear to be occasioned in some way or other by this bed.

Scientific Institution, Princes Street, Cavendish Square.

On Tuesday, Jan. 5, Mr. Singer will begin his course of twelve Lectures on Electricity and Electro-chemical science, which will be continued upon each subsequent Friday and Tuesday, until concluded. And on the 23d of Feb. he will begin his course of Voltaic Electricity. In addition to the extensive apparatus before employed, he has now in forwardness an entire new Battery of one thousand double plates, with a variety of auxiliary apparatus.

Anatomical Theatre; Lower College Street, Bristol.

Mr. Thomas Shute will commence his spring course of Lectures on Anatomy, Physiology, and the principles and operations of Surgery, on Saturday the 8th January, at eight in the morning.

Dr. Buxton will commence his spring Course of Lectures on the Practice of Medicine, about the 20th of January next.

The Pontine Marshes.

It is announced, from the Continent, that the French have succeeded in draining the Pontine Marshes; a pestilential nuisance which has subsisted for so many centuries, in the vicinity of Rome, in defiance of every attempt of the ancient Imperial, as well as of the papal government. This district, once so healthy and so populous, and at length again reclaimed, is said to afford a disposeable quantity of 150,000 acres of excellent land. The means adopted are not, nor perhaps can be, clearly stated in a short notice. That the Engineers have improved the line; regulated the falls; enlarged the water ways; secured the embankments, sluices, and other works; and no doubt, employed the powers of steam to facilitate their general and particular labours—may be concluded from the science and activity of a people, too long employed in the works of destruction. To works like the present every friend to humanity must join in wishing success and duration.

William Davis's Treatise on Land Surveying, to which are now first added a supplement, and a portrait of the Author, the fifth edition greatly improved, enlarged and better arranged is nearly ready for publication.

Mr. Bakewell will commence a course of Lectures on *Geology* and *Mineralogy*, at the Surry Institution, in January, 1813.

Mr. Nicholson takes this opportunity to acquaint his Patrons and Correspondents, that he has been, for some time, occupied upon such arrangements, with regard to his public undertakings and other concerns, as have enabled him to take the conducting and editing of this Journal entirely into his own hands; which, for some time past, have, in a great measure, been committed to an eminent and able scientific gentleman, who is not at present engaged in the work. The whole of the annotations and remarks, together with various original as well as abridged and selected articles, on different subjects, will consequently, as in times past, be produced by Mr. Nicholson; and he looks forward with confidence and pleasure to many a renewed correspondence on the subjects of natural Philosophy and the Arts.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1813.

ARTICLE I.

An Account of some Experiments on different Combinations of Fluoric Acid. By JOHN DAVY, *Esq. From the Philosophical Transactions,* 1812.

Introduction.

Statement of the subject.

TWO years ago, I engaged, at the request of my brother, Sir H. Davy, in an inquiry respecting the nature of common fluoric acid gas. My principal object was to ascertain whether silex is essential to its constitution, and whether the proportion is constantly the same. This subject, and experiments on the fluoric and fluoboracic acids, occupied me for about six months. Since that time, the work of MM. Gay Lussac and Thenard has appeared, entitled "Recherches Physico-Chimiques," in the second volume of which is an elaborate dissertation on fluoric acid. These philosophers, I find, have anticipated many of my results, and consequently very much abridged my labour of detail in the following pages. To repeat what is already known would be useless; I shall therefore confine myself to describe what I have observed, which appears to me yet novel, or different from the observations of the French chemists. The order which I shall pursue, will be that which I observed in my experiments. I shall divide what I have to advance into four parts. The first part will relate to the silicated fluoric acid gas, and to the subsilicated fluoric acid; the second to the combinations of these acids, and of pure fluoric acid with ammonia; the third to fluoboracic acid; and the fourth to its ammoniacal salts.

Sect. 1. *On silicated fluoric acid Gas, and subsilicated fluoric Acid.*

Fluoric acid gas requires either silex or boracic acid to admit of that *state*.

The facts which have already been published by MM. Gay Lussac and Thenard and others, appear to me to be sufficient to prove that pure fluoric acid has not yet been obtained in the gaseous state, and that silex, or boracic acid, is requisite that it may assume this form. Were more evidences necessary, I could advance many in point. One circumstance only I shall mention, proving that common fluoric acid gas is perfectly saturated with silex. I have preserved this gas, made by heating, in a glass retort, a mixture of fluor spar and sulphuric acid, for several weeks over mercury in a glass receiver uncoated with wax, without observing the slightest erosion to be produced.*

Common fl. a. g. is saturated.

It is best obtained by heating fluor spar, finely powdered glass, and sulph. acid together.

This gas, with great propriety, has lately been called silicated fluoric. Before I proceed to its analysis, I shall notice what method I have found the best for obtaining it. I have, for a considerable time, long before MM. Gay Lussac and Thenard's work was published, added to the mixture of fluor spar and sulphuric acid, a quantity of finely pounded glass, and have thus procured the gas with the greatest facility. The advantages of this addition are considerable. The retort is saved, which otherwise, in less than one operation, would be destroyed; and a much larger quantity of gas is procured from the same materials, and with less trouble and less heat; the action indeed at first is so powerful, that gas begins to come over before the application of heat is made, and a very gentle one only is required to continue its production.

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* The sides of the receiver indeed became obscure; but this was not from erosion, but from deposition, as appeared from the transparency and polish of the glass being readily restored by slight friction. What the deposition was, I am ignorant of. After several weeks it was so trifling, as to give only a slight degree of opacity to the receiver.

Previous

Common (or silicated) fl. a. gas is 3½ times heavier than water.

Previous to its analysis, it was necessary to ascertain the specific gravity of the gas. This I have endeavoured to do. The gas, the subject of experiment, was quite pure, being totally condensed by water. A Florence flask was exhausted; in this state, weighed by a very delicate balance, it was

= 1452·2 grains.
Filled with common air - = 1452·2 + 10·2
Again exhausted - - = 1452·2
Filled with silicated fluoric gas = 1452·2 + 36·45

Hence as 10·2 : 31 : : 36·45 : : 110·78

Thus it appears, that 100 cubic inches of silicated fluoric acid gas, at ordinary temperature and pressure, are equal to 110·78 grains.

By precipitating the silex by water and amonia, the gas was found to contain 5 parts acid and 8 silex.

When silicated fluoric acid gas is condensed by water, it is well known that part only of the silex is deposited. To obtain the whole, in order to ascertain the proportion in the gas, I have employed ammonia in excess. 40 cubic inches of the gas (barom. 30, therm. 60) were transferred in portions of 10 cubic inches at a time to a solution of ammonia. The silex precipitated was carefully collected on a filter, and washed till the water that passed through it, ceased to be affected by nitrate of lime. It was next dried, and strongly heated in a platina crucible. It weighed 27·2 grains, and was pure silex. Supposing fluoric acid to be the remaining 17·1 grains, which added to 27·2 grains are equivalent to the weight of 40 cubic inches of the gas, it appears that 100 parts by weight of this gas consist of

61·4 silex
38·6 fluoric acid

100·0

That this estimate may be correct, it is evident, that ammonia should have the property of precipitating the whole of the silex of silicated fluoric gas; which I shall not now endeavour to prove, but leave it to be considered in another part of the paper.

The gas contained no alkali.

There is no improbability attached to the idea, that silicated fluoric acid gas may, from the manner in which it is prepared, contain a proportion of alkali. To discover whether this was the case, a solution of nitrate of lime was added to the ammoniacal solution neutralized by nitric acid, from which the silex in the preceding experiment had been removed. The precipitate of fluat of lime was separated by filtration. The filtered liquid was evaporated to dryness; and the ammoniacal salt heated in a platina crucible till it was entirely dissipated. The residue had the appearance and taste of quick lime. It was dissolved in acetic acid, and the solution yielded sulphat of lime on the addition of sulphat of ammonia. The liquid was evaporated to dryness, and when the residuum has been heated to dull redness, nothing remained but a little white powder, weighing about a grain, and having all the properties of gypsum. Thus it appears that silicated fluoric acid gas contains no alkali.

Common liquid fluoric acid,

My next object was to ascertain the composition of common liquid fluoric acid—that acid obtained by the decomposition of silicated fluoric acid gas by water, and which, on account of the separation that occurs of part of the silex, may, with greater propriety, be called subsilicated fluoric acid. For this purpose, 43·21 cubic inches, barom. 30·4, therm. 60, or 44 cubic inches at common temperature and pressure, were successively added, two cubic inches at a time, to one cubic inch of distilled water in a small jar over mercury. The whole of this, the gas being pure, was readily condensed. The temperature was somewhat raised. The silex precipitated, formed a gelatinous mass of a blueish colour, which had absorbed all the water like a sponge, so that none appeared fluid. This gelatinous mass was carefully transferred to a filter, and washed with distilled water till it was rendered insipid and incapable of reddening litmus paper. It retained its blueish hue only whilst moist. When dried and ignited, it was in thin lamellæ, and of a snow-white colour, and surprisingly bulky. It weighed 7·33 grains, and was found to be pure silex. Thus it appears that the subsilicated fluoric acid formed by the decomposition of 44 cubic inches of silicated fluoric acid gas contains 7·33 grains of silex less than the gas itself. Consequently, independent of water, which no doubt is essential to this acid, 100 parts of it seem to consist of

or subsilicated fluoric acid.

has lost more than one fourth of its silex by uniting with water.

54·56 silex
45·44 acid

100·00

Water condenses 263 times its bulk of sil. fl. a. gas;

I have endeavoured to ascertain what quantity of silicated fluoric acid gas a given quantity of water will condense. In one instance $\frac{19}{100}$ of a cubic inch of distilled water absorbed 51 cubic inches, barom. 30·5, therm. 60. The gas was added to the water in a jar over mercury, as fast as it was absorbed. The experiment was stopped, when the gas, after having remained in contact with the water a whole night, ceased to be diminished. According to this result, the proper correction being made for the additional pressure, water decomposes about 263 times its bulk of silicated fluoric acid gas.

Dr. Priestley observed, that muriatic acid gas re-produced silicated fluoric gas from the crust of silex formed, when the latter is condensed by water*. This experiment I have repeated, and as it appears to show more correctly the quantity of gas water can condense, I shall describe the result. 2·4 cubic inches of muriatic gas were added to a drop of water, that had previously absorbed one cubic inch of silicated fluoric gas, in a jar over mercury. There was an immediate absorption equal to $\frac{3}{10}$ of a cubic inch. The mixture of silex and subsilicated fluoric acid effervesced, and from an apparent solid became fluid, the whole of the silex gradually disappearing. After the first mentioned absorption, there was no farther. The gas produced was silicated, as appeared from the crust it deposited when removed to water, and the liquid formed was pure muriatic acid, for decomposed by concentrated sulphuric, it afforded merely muriatic acid gas, without any silicated fluoric. The evident conclusion from the preceding result s, that water condenses equal quantities of the muriatic and ilicated fluoric acid gases, and consequently that the first stimate is too low, and instead of 263 times its bulk, it is robably more correct to say that water to be saturated requires at least 365 times its volume. Neither will this estimate ppear inconsistent with the former results, when the deposition

or more correctly 365 times.

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* Vide Priestley on Air, Vol. II. p. 202.

tion of silex is considered as an obstacle to the free exposure of the surface of the water to the gas.

Subsilicated fluoric acid is decomposed by alkalies, and by earths and acids.

Subsilicated fluoric acid is decomposed by ammonia and the fixed alkalies, and by all the earths that I have made trial of. It is also decomposed by the sulphuric acid and the boracic, as well as by the muriatic acid gas.

Of the particular changes which occur when it is acted upon by the alkalies, I defer giving any account at present, as it is my intention to do it in the next section.

Habitudes of subsilicated fluoric acid with heat. It came over in distillation, &c.

To learn the effect of heat on it, a small quantity of strong acid, pure and transparent, was introduced into a retort connected with mercury. A spirit lamp being applied, about three cubic inches of silicated fluoric acid gas were produced. The neck of the retort was lined with silex in a gelatinous state, and much liquid subsilicated fluoric acid, that had distilled over, was condensed in the colder part of the neck, and was absorbed by bibulous paper previously introduced, to prevent the distilled fluid from entering the jar for the reception of the gas. When the whole of the acid in the bulb of the retort had been evaporated, little or no silex remained.

This differs from Doctor Priestley's result.

The general result of this experiment is very different from that which Dr. Priestley, who first made it, obtained. Instead of silicated fluoric acid gas, he procured "vitriolic acid air," sulphureous acid gas.

I have tried also the effect of heat on the silicious crust, formed by the decomposition of silicated fluoric acid gas, by water; but could obtain no sulphureous acid gas, as Dr. Priestley did only a small quantity of silicated fluoric.

The correctness of Dr. Priestley's observations cannot be doubted. I can only account for his results, by supposing that some sulphuric acid in consequence of the high temperature employed in making the gas was volatilized, and mixed with the subsilicated fluoric acid, and that mercury also was present, from the acid being prepared over this metal.

Fluoric acid gas cannot be had by distillation free from silex.

These experiments too oppose another statement relative to a method prescribed for making fluoric acid gas free from silex, by merely heating strong subsilicated fluoric acid in a retort, and collecting the gas over mercury. It is asserted, in chemical works of some reputation, that this process is successful. I have never found it so, having always obtained results similar to those above stated. This, I suppose, is one of the many errors that have secretly crept into repute, and has been believed, because never subjected to the test of experiment.

The action of concentrated sulphuric acid on subsilicated fluoric acid, is similar to that of muriatic acid gas, occasioning a disengagement of silicated fluoric acid gas. Facts which appear to prove, that water is absolutely essential to the existence of this acid.

Sulph. acid expels silicated fluoric acid gas from subsilicated fluoric acid.

Boracic acid decomposes it, in a very different way, not from any predominant affinity for the water, but in consequence of a stronger attraction for the fluoric acid itself. Silicated fluoric acid of course is not produced; but liquid fluoboracic acid and the silex is precipitated in a gelatinous state, as when ammonia is employed.

Boracic acid unites with the fluoric acid, and both acids are precipitated along with the silex.

These are the principal facts I have to notice respecting this acid. Before I conclude, I shall briefly mention a few other circumstances. Applied to the tongue, in its concentrated state, it produces a very painful sensation, like that which strong muriatic acid does, and it has a very similar effect on the cuticle. It does not appear to erode glass, for I have kept it in bottles of this substance more than a month without any action being perceptible. Exposed to the air, it slowly and almost completely evaporates, there being only a very trifling silicious residue; and when gently heated in an open vessel, it is rapidly dissipated in white fumes.

Subsilicated fluoric acid acts on the tongue, and does not erode glass.

SECT. II. *On the Combinations of silicated fluoric acid Gas, and the subsilicated Fluoric, and the fluoric Acids with Ammonia.*

M. Gay Lussac has shewn that silicated fluoric acid gas, like carbonic acid gas, condenses twice its volume of the volatile alkali.* The experiment I have several times repeated, and constantly with the same result, no difference appearing when the acid gas was added in great excess to the alkaline, or the alkaline to the acid. This being the case, and knowing the specific gravity of the two gases,† 100 parts by weight of silicated fluat of ammonia seem to consist of

Silicated fluoric acid gas condenses twice its volume, or about one third of its weight of ammonia.

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* Vide Mém. d'Arcueil, Tom. II.

† According to Sir H. Davy, 100 cubic inches of ammonia, barom. 30, therm. 60, weigh 18 grains. It is this estimate which I have taken.

24·5 ammonia
75·5 acid

100·0

Silicated fluat of ammonia volatilizes unaltered, if heated by a spirit-lamp in the vessel in which it is formed, and provided moisture be entirely excluded.

Water decomposes the compound, and precipitates silex.

Like silicated fluoric acid gas itself, this salt is decomposed by water, and a similar precipitation of silex occurs, and in the same proportion. Thus the salt formed by the union of 30 cubic inches of silicated fluoric gas, and 60 of volatile alkali (barom. 30, therm. 60) in a small glass jar over mercury, being carefully collected and introduced into water, afforded five grains of pure silex, weighed after being well washed and heated to redness.

The aqueous solution is subsilicated fluat of silex and ammonia.

The saline solution, since part of the silex of the silicated fluoric acid gas is separated during its production, appears to be a subsilicated fluat, or a combination of subsilicated fluoric acid and ammonia. Another mode of making it, more directly proves that this is its composition. When ammonia is added to the subsilicated fluoric acid in excess, this salt is formed without any precipitation. From these facts, it may be concluded, that independent of water, which appears to be essential to its existence, 100 parts of it consist of

28·34 ammonia
71·66 acid

100·00

It has a pungent taste; reddens litmus; crystallizes not deliquescent; corrodes glass while hot, &c.

Subsilicated fluat of ammonia has a pungent saline taste. It just perceptibly reddens litmus paper. Slowly evaporated, it forms small transparent and brilliant crystals. The largest I could obtain, appeared to be tetrahedral prisms. The solid salt is very soluble in water; but is not deliquescent. When heated it appears to sublime unaltered. It is curious that the solution of this salt, when evaporated by a heat near its boiling point, powerfully erodes the glass or porcelain vessel, and a residuum of silex appears, on the addition of water, to re-dissolve the salt. This erosion and residue of silex I have seen produced three times following, with the same quantity of salt. I mention the fact, which, I believe, was before observed by Scheele, without attempting an explanation of it. It may, perhaps be said, that as the water evaporates, the affinity of the subsilicated fluat for silex increases.

Is decomposed by sulphuric acids and alkalies.

Subsilicated fluat of ammonia is decomposed by the sulphuric acid, and by muriatic acid gas, and also by the fixed alkalies and by ammonia.

Effects of sulphuric acid,

Sulphuric acid expels from it, silicated fluoric gas and hydrated fluoric acid fumes.

and muriatic acid gas.

Muriatic acid gas acts slowly on it, and effects its decomposition apparently through the medium of its water. A little of the crystalline salt was introduced into muriatic acid gas in a jar over mercury. In a short time, some silicated gas was produced, as the silicious deposition, on the addition of water, indicated. Strong muriatic acid was substituted for the acid gas. Now no apparent change took place, for on evaporating the acid, the residue, decomposed by sulphuric acid, afforded only silicated flucric acid gas.

Muriatic acid: no change:

Alkalies form the same salts as with the subsilicated acid.

The alkalies form by the decomposition of this salt, the same compounds that they do by their action on subsilicated fluoric acid.

Potash expels the ammonia, and produces the silicated fluat and fluat of potash, as MM. Gay Lussac and Thenard have described.

The changes occasioned by soda appeared to me similar: but the gentlemen just mentioned, assert that this alkali precipitates the whole of the silex, and does not form a triple salt with it and part of the acid.

Ammonia completely separates the silex.

Ammonia seems to me to separate completely the silex, and by uniting with the pure acid to constitute a true fluat. MM. Gay Lussac and Thenard are of a different opinion. They say that the whole of the silex cannot by this method be removed, but only the principal part. Their reason for this belief, is, that on repeatedly evaporating the salt after the addition of ammonia and re-dissolving it, they have each time observed a residue of silex. If they employed metallic evaporating vessels, the results of my experiments do not agree with theirs; for making use of platina for this purpose, and adding an excess of ammonia, I never detected traces of silex on evaporating the filtered fluat. But our results agree, if they employed glass or porcelain vessels, which fluat of ammonia has the property of corroding.

(*To be Continued*)

II.

Observatians on the Measurement of three Degrees of the Meridian conducted in England by Lieut. Col. William Mudge. By Don JOSEPH RODRIGUEZ. *From the Philosophical Transactions for* 1812. *p.* 321.

(*Concluded from p.* 334, *Vol. XXXIII.*)

The uncertainty which may subsist respecting the figure, &c. of the earth, will affect the entire arc proportionally more than any part of the same; but the contrary happens in the English measurement:

which shews a considerable error of another kind in the obs.

But to return to our subject of the English measurement. If the uncertainty which yet subsists, with respect to the exact figure of the earth and its dimensions, occasions some small errors in the calculation of the series of triangles, the sum of these errors will be found in the estimate of the entire arch, and will increase in proportion to the extent of the arc measured. Now, in the English measurement, we find exactly the reverse of this. For the difference between the results of calculation and observation is only $1'',38$ on the whole arc; but is even as high as $4'',77$ on one of the smaller arcs. So that, whatever error we may suppose to have been introduced into the calculation, by assuming a false estimate of the spheroidity of the earth, or of other elements employed in the calculation, it is very evident that the zenith distances of stars taken at Arbury Hill are affected by some considerable error, wholly independent of these elements.

The errors from uncertainty in the elliptic elements are not considerable.

It was not till the date of the measurement of the meridian in France, that M. Delambre published and explained, with admirable perspicuity and elegance, all the formulæ and methods relative to the calculation of spheroids, and put it in the power of astronomers in general to make use of the elliptic elements in verifying the results of their observations. In the present state of science these elements are well known, and the errors that can arise from any uncertainty in them, are not so considerable as is generally supposed. The oblateness and the diameter at the equator are the only elements wanting in the calculation; for the purpose of seeing what effect our present uncertainty respecting them can have on the subject in question, I have employed three different estimates of the oblateness $\frac{1}{330}$, $\frac{1}{320}$, and $\frac{1}{310}$. With respect to the radius of the equator, that is ascertained with sufficient precision by the mean of the arc extended from Greenwich to Formentera, corresponding to latitude 45° 4′ 18″. The value of the degree in toises is 57010,5, and it is highly probable that in this estimate the error does not amount to so much as half a toise, as it is deduced from an entire arc of 12° 48′ between the two extremities, the latitudes of which have been determined with extreme care, and by a great number of observations.

The following are the logarithms of radius at the equator, which I have employed as adapted to each degree of oblateness, and opposite to them are placed the corresponding computed estimate of the entire arc between Clifton and Dunnose.

By assuming three different estimates of oblateness the differences in the results—

$\frac{1}{33}$ .... 6,5147,400...... 2° 50′ 21,972
$\frac{1}{32}$ .... 6,5147,485...... 2° 50′ 21,974
$\frac{1}{310}$.... 6,5147,570...... 2° 50′ 21,976

so that the greatest difference is but 0″,38. Let us suppose it 0″,4, or even 0″,5, for the second calculation was made only by means of the western series of triangles, and the third only was the eastern; but even then the error arising from uncertainty in the elements is not half the difference we find between the results of computation and of observations of the fixed stars. It appears, therefore, that these elements are by no means to be neglected as a method of verification; and in fact the quantity of 1″,38 is so small, that it is extremely difficult to ascertain this quantity with the very best instruments. Of this we shall find further proof hereafter; but as this discussion is not without its use, I shall enter into some details on this subject.

prove too small to be in general ascertained.

The measurement in Lapland was performed by means of a double metre, and with a repeating circle of Borda, sent by the National Institute of France. In order to see to what degree of accuracy the arc computed would agree with that obtained by observations of the pole star above and below the pole, I assumed an oblateness of $\frac{1}{320}$ and as logarithm of adius I had 6,5147500 expressed in toises and in round numbers.

The same shewn from the Swedish observations;

bers. With these elements, and with the data to be found in the work of M. Svanberg, we have by the western series of triangles 5840″,196 and 5840″,138 by the eastern. So that the mean calculated arc is 1° 37′ 20″, 167, while the arc observed was 1° 37′ 19″,566. The difference then is 0″,6 for the total arc, and 0″,37 for the mean degree, or 5,86 toises excess in the linear extent. One can never depend upon quantities so small as this, so that the agreement between the results of computation and actual observation, proves not only the skill of the observers and the accuracy of which their instruments admit; but also that the elliptic elements employed in the calculation are a sufficiently near approximation to the truth to be deserving of confidence.

and also from the measures on the meridian taken in India by Major William Lambton;

In the 8th volume of the Asiatic Researches, published by the Society at Calcutta, are contained the details of another measurement performed in 1802, by Major William Lambton in Bengal, on the Coromandel coast. In this undertaking, which was executed with great skill and attention, Major Lambton employed Bengal lights as signals, chains for the linear measures, and a theodolite, and a zenith-sector made by Ramsden. The base measured was 6667,740 fathoms reduced to the level of the sea, and to the temperature of 62° Fahrenheit; and the stations were so chosen, that four of the sides of the triangles were almost in the same line, and nearly parallel to the meridian at the southern extremity of the arc, so that their sum but little exceeds its whole extent. The lengths of these arcs in fathoms reduced to the meridian are thus given in the Memoir of Major Lambton.

AB	20758,13	north latitude of A 11° 44′ 52″,59
BC	17481,245	
CD	22237,04	north latitude of E 13° 19′ 49″,018
DE	35246,43	

From these data Major Lambton deduces the degree of the meridian to be 60435 fathoms, or 56762,3 toises. By applying to this the same elements as we did to the measurement by Svanberg, we have the entire arc measured equal to 1° 34′ 55″,896; so that the difference between the results of calculation and of the observations is only 0″,532 for the whole arc, or 0″,337 for the mean degree. The elliptic hypothesis and observation agree more correctly in this instance, for the difference is rather less than in that of Lapland, although the two arcs are very nearly of the same extent. Thus the degree on the meridian measured in Bengal, in the latitude of 12° 32′ 21″ north, cannot be supposed to exceed Major Lambton's estimate by more than 5,22 toises; and it is extremely difficult to speak with certainty to quantities so small as this.

and also from a degree measure by Major Lambton, perpendicular to the meridian.

The same observer also measured one degree perpendicular to the meridian, by means of a large side of one of his triangles cutting the meridian nearly at right angles, and of which he observed the azimuth at the two extremities. The data from which his results may be verified are these:

Length of the chord of the long side in English feet AB = 291197,20.

Azimuth of the eastern extremity A equal to 87° 0′ 7″,54 NW.

Azimuth of the western extremity B equal to 267° 10′ 44″,07 NW.

North latitude of A 12° 32′ 12″,27
North latitude of B 12° 34′ 38″,86.

With these data in the triangle formed by the long side, the meridian at B, and the perpendicular from B on the meridian at A, we have the chord of this last arc equal to 290845,8 feet, and the arc itself 290848,03 feet. By applying the method of M. Delambre, we find the azimuth of the extremity B less by 2″ than it was observed to be; so that we have no reason to suppose a greater error than one second in the observation of each azimuth, and it seems next to impossible to arrive at a greater exactness.

The difference of longitude between the points A and B is 48′ 57″,36. With this angle and the co-latitude at A, we have in the spherical triangle right angled at the point A, the extent of the normal arc equal to 2867,330 seconds, and dividing its length in feet by this number, we have for the degree perpendicular to the meridian, at the extremity A, 60861,20 fathoms, or 57106,5 toises. Now these values are precisely what we find on the elliptic hypothesis, with an oblateness of $\frac{1}{340}$ or $\frac{1}{310}$; and in short, the correspondence between the hypothesis and the measures of Major Lambton, is as complete as can be wished. Major Lambton, indeed, finds the degree on the perpendicular too great by 200 fathoms, but this arises from a mistake in his calculation.

By applying the same method of computing the arcs in seconds and in toises, the elliptic hypothesis is found to agree with the late measures taken in France.

Lastly, I shall apply the same method, and see how nearly the elliptic hypothesis agrees with the last measures taken in France, which merit the highest degree of confidence, both with respect to the observers who have executed it, and the means which they had it in their power to employ. I have taken only the arc between Dunkirk and the Pantheon at Paris, from the data published by the Chevalier Delambre in the 3d vol. of the Measurement of the Meridian. I employed the same elements and similar calculations to those made on the English arc. The oblateness of $\frac{1}{310}$ gives the difference between the parallels equal to 7883,615 seconds by the eastern series of triangles, and 7883,617 by the western series. The mean of these 7883,616 may be taken as the true extent of the total arc.

The two other elements give for this quantity, 7883",621 and 7883",493, or 2° 11' 23",6 and 23",49, as the calculated extent of the arc. But the arc observed was 2° 11' 19",83, according to M. Delambre, and 2° 11' 20",85 according to M. Mechain; so that the least difference between the calculation and the observations will be 2",64. M. Delambre is of opinion, that the latitude of Dunkirk, which is supposed to be 51° 2' 9",20, should be diminished; and in fact the distance between the parallels of Dunkirk and Greenwich, which is 25241,9 toises, gives by the mean of the three assumed elliptiticies 26' 32",3 for the difference of latitude. After deducting this quantity from 51° 28' 40", the supposed latitude of Greenwich, there remains 51° 2' 7",7 or 8", for that of the tower at Dunkirk. If from this again we deduct the calculated arc 2° 11' 23",5, we have 48° 50' 44",5 for the latitude of the Pantheon, while, according to the observations of M. Delambre, it is 49",37, or 48",35 by those of M. Mechain. If various circumstances, with regard to unfavourable weather, and also others of a different kind connected with the revolution, and of which M. Delambre complains with much reason, have occasioned some uncertainty with respect to the observations at Dunkirk, still the numerous observations made at Paris, both by him and by M. Mechain at a more favourable season, and in times of perfect tranquillity, render the supposition of an error of 4 seconds in the latitude of the

Pantheon wholly inadmissible. It is, however, too true, that such errors are possible, and it is only by careful perseverance, and by repeated verification, that they are to be discovered and removed, as we have seen to be highly probable with respect to the station at Arbury Hill.

But the same celebrated observer, M. Mechain, who handled instruments with great delicacy, and was possessed of peculiar talents for this species of observation, has given us an instance of singular irregularity in the observations made at Montjui and at Barcelona.

Instance of irregularity in observing the difference of latitude

The latitude of Montjui, determined by a very long and regular series of zenith distances, is full 3″24 less than that deduced from a similar series of observations made at Barcelona, with the very same instruments, and with equal care. Moreover, there is reason to think, from other observations, that the latitude of Barcelona (which is supposed to be 45″) ought to be diminished still one second, so that the difference between the observations at Montjui and at Barcelona, will probably amount to as much as 4″. Local attractions are supposed to have been the cause of this irregularity; but then the latitude, as deduced from observations made at Barcelona, should have been less than it appeared by those made at Montjui itself; for the deviation of the plumb-line (or of the spirit contained in a level) *could only* be occasioned by the little chain of land elevated to 120 or 130 toises, which passes to the north of Barcelona in a north-easterly direction. Now since the deviations arising from this source would be northward, the zenith distance of circumpolar stars would be augmented by that deviation, and consequently the latitude deduced therefrom would be diminished by just so much. But here the contrary occurs; for the latitude of Montjui deduced from the observations at Barcelona is 48″,23, whilst that obtained by direct observations at Montjui is only 45″. Hence it seems probable, that the cause of this irregularity must be sought elsewhere, and that it is not likely to be discovered without repeating over again the same observations.

between Barcelona and Montjui:

ascribed to local attractions;

but the deviations are of a contrary nature.

Moreover it does not follow that the latitudes of two places are correct, because the declinations of the stars deduced from them correspond; for the deviations caused by local attractions, or from any other source, are made to disappear in correcting the declination, but remain uncorrected in the latitude of each.

It is more probable that the error on the English arc may be in the observations.

Lieut. Col. Mudge is also of opinion, that the irregularity in the value of his degree may be ascribed to deviation of the plumb-line, occasioned by local attractions. This is certainly very possible, and may be decided by an examination of all circumstances on the spot. But if there be really an error of 1'' in the extent of the whole arc, this should rather be ascribed to some defect in the observations themselves, than to any extraneous source; for the observations of different stars give results that differ more than 4 seconds from each other.

Measurements still wanting in the southern hemisphere.

I shall now conclude this Memoir, by expressing a wish, which men of science in England have it more in their power than any others to gratify; I mean by making new measurements in the southern hemisphere. Those which have been made hitherto in the northern hemisphere are extremely satisfactory by their agreement, and give us great reason to presume that the general level of the earth's surface is elliptical, and very regularly so; and hence we might expect the opposite hemisphere to be equally so, and to be a portion of the same curve. Nevertheless, the degree measured at the Cape of Good Hope by Lacaille, in latitude 33° 18′ appears to indicate an ellipse of less eccentricity, or of greater axis; for the linear extent of 57037 toises, corresponds to the measure of a degree in latitude 47° 47′ in the northern hemisphere. If now we calculate the arc as before, with an oblateness of $\frac{1}{320}$, and with the sides of Lacaille's triangles reduced to the meridian, we find it greater by 10″ than it was found to be by observations of the stars. An error of 10 seconds, by an astronomer so skilful and scrupulous as Lacaille, is too extraordinary to be admitted as probable. It is true, that there was a greater error well ascertained to have occurred in the measurement in Lapland, amounting to 13 seconds; but the academicians engaged in this undertaking were by no means equally conversant with observations as Lacaille.

For the measures of Lacaille seem to indicate an ellipse of less eccentricity.

Proposed admeasurement at the Cape,

There remains, therefore, but one method of removing all doubt on this subject, and this is to repeat and verify the measurement at the Cape, and, if possible, to extend it still farther to the north. The same Major Lambton, who has succeeded so well in Asia, and is in possession of such perfect instruments for the purpose, would be singularly qualified for a similar undertaking in Africa, and would furnish us with a measurement in the other hemisphere, as much to be relied upon as the former. He would have the glory of deciding two important questions by his own observations; first, the similarity and magnitude of the two hemispheres: and, secondly, the degree of reliance to be placed on the elliptic hypothesis.

and in New Holland.

It might be still further desirable, if other measurements could also be undertaken, either in New Holland, or in Brazil; for though neither of these countries differs much in latitude from the Cape of Good Hope, they are so remote in longitude, that a correspondence of measures so taken would nearly establish the similarity of all meridians.

Note.

Formulæ employed in the preceding computation.

I shall now explain the formulæ employed in deducing the results to which I have come in the foregoing Memoir. The demonstration of them is to be found in the work of M. Delambre, on the Meridian.

In the first place, let a be the radius of the equator, e the eccentricity, ψ the latitude of one extremity of a side, or arc, in any series of triangles, and θ the azimuth of that side. The radius of curvature of this arc will be expressed by

$$\frac{1}{R1} = \frac{\left(1 + \frac{e^2}{1-e^2} \cdot \cos.^2\psi \,.\, \cos.^2\theta\right)}{R} \text{ and } \frac{1}{R} = \frac{(1-e^2 \,.\, \sin.^2\psi)^{\frac{1}{2}}}{a}.$$

Hence we see that R is the radius of the arc at right angles to the meridian. One may in general neglect the azimuth, and take the last radius for the radius R1. Now, in computing the arc between Clifton and Dunnose, I have supposed the oblateness to be $\frac{1}{330}$ or $e^2 = \frac{669}{330^2}$, and log. $a = 6{,}5147200$ expressed in toises.

The latitude of the southern extremity of the base is the same as that of Clifton, and its azimuth, if we choose to attend to it, is nearly 335° 23′. This base, considered as an arc of a circle, is reduced to its sine by the formula $s = \log. s - \frac{K \,.\, s^3}{6R^2}$, Formulæ employed in the preceding computation.

(K being the modules of the table of logarithms, so that log. K=9,6377843.)

By means of the logarithmic sine of the base, and the angles of the triangles, considered as spherical, the logarithmic sines of the sides in the series were next computed, and then reduced to logarithms of the arcs themselves by the formula $\log. s = \log. \sin. s + \frac{K. \sin.^2 s}{6R^2}$.

For the purpose of making this last reduction, it is sufficient to take a single value of R, corresponding to the mean latitude of the entire arc 52° 2′ 20″. It was thus that the table was formed of logarithmic sides considered as arcs.

Let m be one of these arcs, and let us represent by $\delta\psi$ and $\delta\psi''$ its value reduced to the meridian, the one in toises, the other in seconds of a degree, and we shall have the following formulæ;

$$\delta\psi = m . \cos. \theta - \left(\frac{m^2 . \sin,^2 \theta}{2R}\right) . \text{tang.} \psi - \left(\frac{m^2 . \sin^2 \theta}{2R}\right) . \left(\frac{m . \cos. \theta}{3R}\right) . (1 + 3 \tan.^2 \psi)$$

$$\delta\psi'' = \left(\frac{\delta\psi}{R . \sin. 1''}\right) + \left(\frac{\delta\psi}{R. \sin, 1'}\right) . e^2 . (1 + e^2) : \cos.^2 \psi . \left\{1 \mp \left(\frac{3 \tan. \psi}{2}\right) . \left(\frac{\delta\psi}{R}\right)\right\}$$

: the superior sign being taken when the latitude ψ'' is greater than ψ, and the inferior when it is less.

The correction dependent on the convergence of the meridian for the azimuths is $\delta\theta = \left(\frac{m . \sin. \theta}{R1 . \sin. 1''}\right) . \left(\frac{\sin. \frac{1}{2}(\psi' + \psi)}{\cos. \psi' . \cos. \frac{1}{2} \delta\psi''}\right)$.

Hence the azimuth of the first station seen from the second and reckoned westward from the north, is $\theta' = 180° + \theta + \delta\theta$.

If P″ be put for the difference of longitude between two points distant by an arc which measures m, we have $\sin. P = \frac{\sin. m . \sin. \theta}{\cos. \psi}$, $\log. \sin. m = \log. \left(\frac{m}{R1}\right) - \frac{K}{6} . \left(\frac{m}{R1}\right)^2$, and $\log. P'' = \log. \left(\frac{\sin. P''}{\sin 1''}\right) + \frac{6}{K} . (\sin. P''.)$

The arc of the meridian, between Greenwich and Formentera, is so fortunately situated, that its middle point is in latitude 45°. Its whole extent measures 12° 48′ 44″, and the distance between the parallels, in linear measure, was found to be 730430,7 toises. Hence the mean degree, corresponding to the latitude of 45° 4′ 18″, is 57010,5 toises; and if we multiply this number by 90°, we get one-fourth part of the meridian of the earth.

Formulæ employed in the preceding computation.

The correction to be deduced for oblateness is 58, 59, or 61 toises, according as it is assumed to be $\frac{1}{330}$, $\frac{1}{320}$, or $\frac{1}{310}$, and if we take the mean of these, we have the fourth part of the meridian Q = 5130886 toises; and hence the metre = 44330867 lines; so that the value of the metre turns out to be almost entirely independent of the elliptical form of the earth.

The radius of the equator is derived from the expression $\log. a = \log. \left(\frac{2Q}{\pi}\right) + K . (\frac{1}{2} . \epsilon + \frac{1}{16} . \epsilon^2 - \frac{1}{48} . \epsilon^3)$, ϵ being the oblateness, and π the periphery of a circle = 3,1416.

In order to compare any degrees measured with those obtained on the elliptic hypothesis, we have a very simple formula. Let m and m' be the values of two degrees on the meridian, of which the mean latitudes are $\psi 1$ and $\psi 2$; in comparing the analytic expressions for these two degrees developing them, and then making $\psi = 45°$, we have $m' = m . (1 - \frac{1}{2} . p . \cos. 2\psi 2 + g . \cos. ^2 2\psi 2)$, $m = 57010{,}5$ toises, $p = \frac{3}{8} e^2 . (1 + \frac{1}{2} e^2) : \frac{\sin. 1°}{1 . \sin. 1'}$, and $g = \frac{15}{64} . e^4 . \left(\frac{\sin. 2°}{1° . \sin. 1''}\right)$

And then we shall find that the oblateness $\frac{1}{334}$ gives 57075,66 and 57192,38 toises for the degrees in England and Lapland.

I shall here subjoin one reflection more, which appears of importance. The oblateness of the earth is a quantity which varies considerably, by the least difference in the elements on which it depends. Accordingly, it is not surprising, that its value fluctuates between two proportions which differ sensibly from each other. To illustrate this, let p be the function which serves to determine the oblateness of the earth, so that $\frac{1}{\epsilon} = p$. When this equation varies $- \delta\epsilon = \epsilon^2 . \delta p$.

Now the coefficient ϵ^2 being very great, we see why the least variation in the elements of the function p, occasions so considerable a variation in the denominator of the oblateness. This is precisely what happens in the lunar equations dependent on the figure of the earth, and which M. Laplace has deduced from his beautiful theory. Thus, for example, in the inequality that depends on the longitude of the moon's node, which he has determined analytically with so much precision, the numerical coefficient found by Burg gives $\frac{1}{305}$ for the oblateness; but if this coefficient be diminished by 0″,665, then the oblateness becomes $\frac{1}{320}$, so that a variation even to this small amount in the coefficient augments the denominator of the oblateness nearly $\frac{1}{20}$ part.

The same happens with regard to the pendulum vibrating seconds; for, supposing its length at 45° to have been correctly ascertained by MM. Biot and Mathieu, if we wish to know the length of a second's pendulum at the equator, corresponding to an oblateness of $\frac{1}{320}$, we find it to be 439,1810 lines. Now this length differs from that determined by Bouguer only by 0,029 of a line, and M. Laplace even thinks that the result of Bouguer should be diminished by about double this quantity. We see from hence how much these little differences, whether produced by errors of observation, or irregularities in the earth itself, are liable to affect the denominator of the fraction expressing the oblateness.

Fortunately, it seems probable, that the utmost latitude of our present uncertainty is between the limits of 330 and 310, and the mean of these may be considered as a very near approximation to the truth.

III.

Critical Observations, on Dr. Wollaston's stated improvement of the Camera Obscura and Microscope in the application of the Meniscus, and Two Plano-Convex Lenses; proving their inferiority to the double Convex Lens generally used. By Mr. WILLIAM JONES, *Optician.*

To Mr. Nicholson.

SIR,

Reference to a former of the author upon periscopic spectacles.

AS my observations, published in your *Journal*, Volume 7, proved, I trust, that the Periscopic Spectacle Glass, advertised by Dr. Wollaston, as possessing a new optical principle,

ple, and affording an improvement in the figure of a spectacle glass, was no other than the old rejected Meniscus Lens; contained no principle of refraction, different from the plano-convex, and double convex lenses; but, as it caused a greater aberration of the rays of light than those two lenses, was a worse form of lens for spectacles, or any other instrument, than the double convex lens generally used by practical opticians: It must, therefore, surprise others, besides myself, that Dr. W. should be induced again to propose the Meniscus, as an improvement in the Camera Obscura, by substituting it for the double convex lens, his account of which you copied last month into your Journal, from the Philosophical Transactions for 1812.

Motive of the present memoir.

The desire I have to maintain an optical truth, and the duty I owe to our professional interest, obliges me again to point out to your readers, what I judge to be the error of his reasoning, and the fallacy of the inference.

Remedy stated by Dr. Wollaston as applicable to the common Camera; viz. to place the screen within the principal focus.

In his description of the effect of the double convex lens in the common camera, page 27, he states the known effect of the images distant from the middle, or direct focus of the lens, being somewhat indistinct, on account of the plane of representation becoming, in distance, greater than the principal focus of the lens; and the oblique pencils of rays being refracted to a focus, rather shorter than the principal one. "On this account," he adds, "it is in general best to place the lens at a distance somewhat less than that which would give most distinctness to the central images, because in that case a certain moderate extension is given to the field of view, from an adjustment better adapted to lateral objects, without materially impairing the brightness of those in the centre." The aberrations of the lens add also to the indistinctness.

Objection, and description of the remedy in use; viz. to make the screen concave.

The collateral indistinctness in our portable chest Cameras, is but trivial and unimportant; and, in my opinion, the remedy, as above proposed, will be found by the artist to be worse than the defect, as the distinct and vivid central images will be vitiated, and the extreme images but little improved. The most perfect remedy is that which has been used by opticians in large cameras, for more than 50 years past, of placing a bottom board, or whitened table, with a concave surface, proportionated to the focal distance of the lens; which, corresponding very nearly to the focus of all oblique refracted rays, exhibits universally the images with the greatest brilliancy and distinctness.

The exact curve of the surface of this board or table should be that of a conic section, but the concave (spherical) one answers sufficiently well. It is necessary for the reader unskilled in optics to know, that what opticians name the axis of a lens, is that imaginary line that is supposed to pass through its centre, and is not subject to any refraction, and all other rays incident on the surface are refrangible, in proportion to the angle they make with this axis, those rays impinging nearest the centre of the lens, and with the least obliquity of position, are refracted with the most perfect images, or with the least aberration, in double convex, plano-convex, and meniscus lenses. The longitudinal aberration produces a focus short of the principal one, and the lateral aberration a confused lateral extension of the images, blended with prismatic colour. These aberrations increase directly with the diameter and thickness of the lens, and inversely with its focus. In lenses of large diameter, and short foci, these aberrations will, by experiment, be rendered very manifest, and which have been clearly demonstrated by that learned optician, Mr. Benjamin Martin, in his Elements of Optics, Dr. Smith, and others.

Dr. Wollaston's improvement, with the meniscus described by quotation.

The subsequent paragraph, page 27, describes Dr. Wollaston's proposed improvement: the substance, in his own words, is as follows. "The lens is a meniscus, with the curvatures of its surfaces about in the proportion of two to one, so placed, that its concavity is presented to the object, and its convexity toward the plane on which the images are formed. The aperture of the lens is four inches, its focus about twenty-two. There is also a circular opening, two inches in diameter, placed at about one-eighth of the focal length of the lens from its concave side, as the means of determining the quantity and direction of rays that are to be transmitted. The advantage of this construction over the common camera obscura is such, that no one who makes the comparison can doubt of its superiority; but the causes of this may require some explanation. It has been already observed, that by the common lens any oblique pencil of rays is brought to a focus at a distance less than that of the principal focus. But in the construction above described, the focal distance of oblique pencils is not merely as great, but is greater than that of a direct pencil. For, since the effect of the first surface is to occasion divergence of parallel rays, and thereby to elongate the focus ultimately produced by the second surface, and since the degree of that divergence is increased by obliquity of incidence, the focal length resulting from the combined action of both surfaces will be greater than in the centre, if the incidence on the second surface be not so oblique as to increase the convergence. On this account, the opening E is placed so much nearer to the lens than the centre of its second surface, that oblique rays E*f*, after being refracted at the first surface, are transmitted through the lens nearly in the direction of its shorter radius; and hence are made to converge to a point so distant, that the image (at *f*) falls very nearly in the same plane with that of an object centrally placed."

Observations to shew that the meniscus is inferior in its effect to the double convex lens.

The radii of curvatures for a meniscus of 22 inches focus, being as two to one, is not essential. The theory of dioptrics shews, the greater the proportion, or the nearer that the radius of one side approaches to infinity, or a plano, the more perfect the lens will be. Dr. W. has not stated the diameter of the convex lens, but the reader must suppose it to be four inches, like that of the meniscus; nor has he told the reader what improvement would be produced, if he placed a similar circular opening, or limited aperture, also over the convex lens. I must, therefore, inform the reader, and he may himself prove it to be correct. The diameter or aperture of four inches is too great for a lens of 22 inches focus, either double convex, or meniscus lens, placed in a Camera-Obscura, as it transmits too much light, and produces too much aberration for the most distinct representation of the images within the Camera. Dr. W., therefore, no doubt, was obliged to correct this palpable defect, by a curtailment of the area of his lens no less than *three-fourths* of the whole, and the lens would have been more like one applied by a skilful optician, if he had at first inserted a lens of about two inches diameter. The limited aperture, therefore, it is evident, advantageously excludes superfluous rays, but has nothing to do with the *determination* of their *direction*. Upon a fair comparison, the reader will not only doubt of the superiority of Dr. W.'s Camera, but be *convinced* of its absolute inferiority; for the double convex lens, under the

same

Observations to shew that the meniscus is inferior in its effect to the double convex lens.

same diameter and focus as the meniscus, has less spherical surface, and consequently less longitudinal and lateral aberration of the two. Let us now advert to the transformation of the convex lens to become a meniscus, with the same focus; by considering their figures in the diagrams, the reader will perceive, that as much as the upper surface of the convex has been incurvated for a meniscus, so much the more has the convexity of the under side been augmented, to retain the original focus. The oblique pencils of rays first entering the meniscus, or any part of its surface, are from the immutable law of refraction refracted from the axis of the lens, contrarywise to the first direction on the convex, and afterwards in their passage into air, by the increased inferior convexity, refracted back towards the axis proportionally more than by the under side of the double convex to be converged to the same focal distance; and all pencils of rays that impinge on the surface in an oblique direction to its axis, must be united the same as by the convex lens, at a focus somewhat shorter than the principal focus from direct rays. The meniscus lens, in refractive property, differs not from the double convex one. The above explanation is agreeable to all writers on optics, and to correct experiment. In this meniscus, it is not "IF the incidence," &c. but the incidence *always* is so oblique on the second surface, as to increase the convergence; and no kind of opening E whatever will change nature's laws of refraction, so as to elongate the focus, or to produce two different focuses in one lens; and his previous explanation of "occasioning all pencils to pass, as nearly as may be, at right angles to the surfaces of the lens," page 27, is an irrelevancy in optics, and is the error of reasoning that I imputed formerly to Dr. W. on his spectacle glass. It is the angle that the rays make with the *axis of the lens*, of whatever shape, that refraction is estimated from, as the science teaches us; not from the geometrical positions of pencils and surfaces. From the greater aberration that the meniscus possesses, the images formed by it will be less distinct, have less light, and be more distorted than by the double convex lens. It is from the extended lateral distortion, and bringing the meniscus nearer to the plane than its exact focus, that I can assign a cause how Dr. W. could have fallen into the error; had he placed the concave side downwards, it would have been a better position, the images would have been more defined and enlightened; it was so applied in his spectacles, the convex side being next to the object: but in neither case will the images be so perfect and vivid, as by the double convex lens. The meniscus in a Camera is not a new application; several, some years back, were made for the purpose, but not preferred. I can refer to the machine now existing with one. I have caused two lenses to be ground, one a double convex, the other a meniscus, as Dr. W. directs, of the same diameter, nearly four inches, and focus twenty-two inches; which experimentally verify the correctness of my observations, and which any intelligent person may inspect, by application at our manufactory, 30, Holborn.

The meniscus in a Camera not new. Reference to experiment.

The following quotations may to some of your readers better corroborate the truth of my remarks.

Quotations respecting lenses.

"If the side were concave (of a plano) so that the lens became a meniscus, there is no proportion of the radii, or position of the lens, with regard to the radiant, but what will give the aberration greater than the plano convex in its best position; and, since this was first observed by opticians, the meniscus began to lose ground in the construction of optical instruments, and is now quite rejected." Martin's Elements of Optics, 1759, page 29.

An oblique pencil of rays has its focus a little nearer the lens (double convex) than a direct pencil. Cor. fig. 2.

This prop. holds good in a concave lens, and also in a meniscus, as well as in a convex one. Emerson's Optics, page 124, prop. 24.

"When parallel rays fall upon the plane side of a plano-convex glass, the aberration of the extreme ray, which is $\frac{9}{2}$ of the thickness, is less than the like aberration caused by any meniscus glass whose concave side is exposed to the incident ray.

"When the said glasses have their convexities turned to the incident rays, the aberration of the extreme ray in the plano-convex, which is now but $\frac{7}{6}$ of its thickness, is less than the like aberration of any meniscus in this position."

The best of all double concave glasses has the semi-diameters of its first and second concavities as 1 to 6; and consequently, this is the best figure of a glass to help short-sighted persons, as the double convex, one of the like figure is the best for spectacles." Smith's Optics, Art. 661, 662. 665.

"For since a meniscus, unless the surfaces of it are parallel to one another, has the same effect either that a convex lens, or a concave one would have, all the cases of diverging or converging rays that are refracted by it, will be the same with those already explained in the instances of convex or concave lenses." Rutherford's Philos. vol. 1, page 286.

"A plano-convex glass, with its $\left\{\begin{matrix}\text{convex}\\ \text{plane}\end{matrix}\right\}$ side towards the incident parallel rays, has less aberration than any meniscus with its $\left\{\begin{matrix}\text{convex}\\ \text{concave}\end{matrix}\right\}$ side exposed to parallel rays. Whence it necessarily follows, that that meniscus is best, which approaches nearest in shape to a plano-convex lens." Harris (of the Mint) Optics, 1776, p. 67.

So sensible have some optical glass grinders been of the impracticability and insufficiency of the meniscus glasses of short foci for spectacles, that I have in my possession some plano-convex and plano-concave glasses actually fitted in the frames, and sold for the *new periscopic glasses*.

Observations on the periscopic microscope.

The sort of French angle of reduction that Dr. W. has given, to obtain geometrically but nearly the radii of meniscus for a given focus, will be useless to the workman, as he already knows, by a very short arithmetical operation, how to obtain exactly such radii in half a minute's time, or a tenth part of the time necessary to construct that problem by Gunter's sliding rule, the time would be still shorter.

The combination of using two glasses in ordinary simple microscopes, or hand magnifiers, to diminish the errors arising from the spherical figure of one glass, was known to Sir Isaac Newton, and successive opticians. That late excellent practical optician, Mr. Ramsden, by the combination in the best position of two plano glasses, with their convex sides to each other, applied eye-pieces to his instruments with great advantage, to read off divisions of his circles, and magnify the wires of his telescopes, with clear definition at the circumference of the field of view, the diameters of the glasses being no smaller than the aperture of the tube. The same principle has since been advantageously applied to large object lenses for the lucernal microscope,

scope, by the late Mr. G. Adams, and ourselves, where the diminution of light was of less consequence than indistinctness of the image In many cases the combination of two convex lenses answer very well: but the combining of two similar plano-convex lenses together, of superfluous diameter and thickness, and for the greatest defect or aberration in the worst position to each other; and afterwards to palliate it with a small aperture as shewn in figure 4, is such an anomaly or absurdity in opitics as not to require any serious comment on my part. I shall only appeal to the least experienced constructor of microscopes, whether he does not know, that the substitution of a double convex lens of the diameter only of Dr W.'s aperture, and of the same focus, would produce an image infinitely more perfect and vivid than the mutilated one proposed by Dr. W.

From these remarks I presume there will be nothing to apprehend from the attempt of Dr. W. to depreciate the excellence of the spectacles, Camera Obsuras, and Mocroscopes, as have been constructed by the most eminent Opticians of the day.

I am, Sir,
Your's, &c.
W. JONES.

Holborn, 16*th Jan.* 1813.

IV.

Rules for discovering new Improvements, exemplified in the art of thrashing and cleaning grain; hulling rice; warming rooms; preventing ships from sinking, &c. By OLIVER EVANS, *of Philadelphia**.

Origin of Inventions.

NECESSITY is called the mother of inventions; but upon inquiry we shall find that reason and experiment bring them forth; for almost all inventions have been discovered by such steps as the following; which may be taken as a

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* From the Appendix to his "Young Mill-wright and Miller's Guide" printed by subscription in Philadelphia, but very sarce in this country.

Rule

Rule for inventing I. Consider the theory and present practice of art. II. and what in speculation would be the best plan of operating: III. how far and in what respects the present practice can be improved: IV. make experiments or Trials of the plans thus deduced from reasoning.

Step I. Is to investigate the fundamental principles of the theory, and the process of the art or manufacture we wish to improve.

II. To consider what is the best plan in theory that can be deduced from, or founded on, these principles, to produce the effect we desire.

III Consider whether the theory is already put in practice to the best advantage, and what are the imperfections or disadvantages of the common process of the art, and whether they can be evaded and the process improved; and what plans are most likely to succeed.

IV. Make experiments in practice to try any plans that the speculative reasonings may propose or lead to. Any ingenious artist, taking the foregoing steps, will probably be led to improvement in his own art; for we see by daily experience that every art may be improved. It will, however, be in vain to attempt improvements, unless the mind be freed from prejudice in favour of established plans.

Example. I. *Thrashing of grain.*

Example 1. Suppose we take the art of thrashing grain.

Then by the rule.

I. *Principles of the art.* To disengage the grain: by beating or by rubbing.

Step 1. What are the principles on which this art is founded? The grain is contained in a head on the top of the straw enclosed in a husk, or chaff, that requires a force to break the hull, and disengage it; which may be done either on the principle of beating or of rubbing.

II. *Theory* apply force to the heads only.

II. What is the best plan in theory for effecting this? As we find that it requires nearly equal force, and is all contained in the head, which is much less in quantity than the straw, theory directs the force to be regularly and uniformly applied to the head only, which will require but little power, seeing we can rub it out between our hands.

III. *Present practice.* Thrashing by men: treading by animals.

III. How is this theory put in practice; and what are the imperfections and disadvantages of the common process? the grain in the straw is laid on a plank floor, and beaten by men, with flails; or on the ground, and trod out by horses.

Disadvantages. I. The force is employed on the straw as

The disadvantages are.

1st. The force is in both cases applied equally to the straw, as well as the head.

II. Much force is lost, being unnecessarily expended in beating the straw, yet many heads escape undone, because the force is so irregularly applied.

well as the head; II. and force lost.

III. In treading by horses the grain as well as the straw gets dirty.

III. Cattle make grain dirty.

IV. Thrashing by men is both expensive and tedious. Now cannot improvements be made to overcome all these disadvantages? Such speculations have produced several.

IV. Hand Thrashing is expensive and slow.

First, a machine on the principles of a coffee mill, which requires very little force to rub the grain out of the heads, which are separated from the straw, by means of a machine on the principle of a comb, cutting them off. A machine to reap the heads without the straw is wanted to complete this theory, (in countries where the straw itself is not an article of demand).

Machines already made.

(a) A mill to rub the grain from the heads.

Secondly, a machine invented and put in practice by Colonel Alexander Anderson, of Philadelphia; the principles of which are to apply the strength of horses to strike the straw regularly with a uniform force, which finishes as it goes and clears the grain at the same time.

(b) Revolving Cylinder with beaters on its circumference, commonly used in England.

A Cylinder, 4 feet long, and 3 feet 6 inches diameter, with eight bats fastened to its circumference parallel to it axis, and of its whole length, is made to revolve with rapidity; the bats strike the straw at every fourth of an inch, it being drawn into the machine by and between two collars that move slowly. This machine makes great dispatch, but is expensive. (and destroys the straw.)

Others, attending to the principles of treading, have made a thing in the form of the frustrum of a cane or sugar loaf, set full of cogs to act like the horses' feet. This is drawn by horses round a circular floor, adapted to it, on which the grain is laid, the centre of the circle being the vertex of the cone. This having considerable weight and many cogs, a horse will beat out much more with it than with his feet, because it will strike a great many more strokes with equal force: it has these advantages; it can be made by an ordinary carpenter—is cheap—and the dirt is not mixed with the grain, straw, &c.

(c) a rolling cone with cogs or studs.

Example II. The art of cleaning grain by wind.

By the rule.

Example II *Art of winnowing.*

Step 1. what are the principles on which the art is founded Bodies falling through resisting mediums, their velocities are as

Its principle. Light bodies

are acted upon by the air more than heavy ones.

their specific gravities; consequently, the farther they fall the greater will be their distance; on this principle a separation can be effected.

Practical result. Blow the light bodies from the grain, by a current of air; deep; but not wide, across the stream. Let the grain &c. fall into compartments and use the same blast repeatedly.

II. What is the best plan in theory? First make a current of air for the grain to fall through, as deep as possible. Then the lightest will be carried farthest and the separation be more complete at the end of the fall. Secondly, cause the grain, with the chaff, &c. to fall in a narrow line across the current, that the light parts may meet no obstruction from the heavy in being carried forward. Thirdly, fix a moveable board edgeways to separate between the good clean grain and light grain &c. Fourthly cause the same blast to blow the grain several times, and thereby effect a complete separation at one operation.

Present practice. The grain does not fall through a suitable cavity; nor is it cleaned at one operation.

III. Is this theory in practice already, and what are the disadvantages of the common process? We find that the common farmers' fans drop the grain in a line 15 inches wide, to fall through a current of air about 8 inches deep, (instead of falling in a line $\frac{1}{2}$ an inch wide through a current 3 feet deep) so that it requires a very strong blast even to blow out the chaff; but garlic, light grains, &c. cannot be got out, they meet with so much obstruction from the heavy grain. It has to undergo 2 or 3 operations; so that the practice is found to be no way equal to the theory; and appears absurd when tried by the scale of reason.

Plan of improvements.

IV. The fourth step is to construct a fan to put the theory in practice, to try the experiment*.

Exp. III. *Art of warming apartments.*

Example III. The art of warming rooms by fire.

Nature of fire not discussed.

Step I. The principles of fire are too mysterious to be investigated here; but the effects are,

Effects I It rarefies the air.

1st. The fire rarefies the air in the room, which gives the sensation of heat or warmth.

II. Causes part to ascend;

II. The warmest part being lightest, rises to the uppermost part of the room, and will ascend through holes, (if there be any) into the room above, making it warmer than the one in which the fire is.

III. particularly in the chimney, which pro-

III. If the chimney be warm, the air will fly up it first, leaving the room empty. The cold air will then rush in at all crevices to supply its place which keeps the room cold.

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* This Machinery, with a large passage or channel, is useful to clean feathers from dirt and heavy bodies.—W. N.

duces cold drafts.

II. Considering the principles, what is the best plan in theory for warming a room?

Practical result.

I. We must contrive the fire to spend all its heat to warm the air as it comes in the room.

I. Employ the whole heat in warming the air.

II. To retain the warm air in the rooms, and let the coldest out first to obtain a ventilation.

II Prevent its escape, and ventilate by escape of cold air.

III. Make the fire in a lower room, conducting the heat through the floor into the upper one, and leaving another hole for the cold air to descend to the lower room.

III. Warm and ventilate several apartments

IV. Make the room perfectly tight so as to admit no cold air, but all warmed as it comes in.

IV. heat the air as it comes in.

V. By stopping up the chimney to let no warm air escape up it, but what is absolutely necessary to kindle the fire, a hole of two square inches will be sufficient for a very large room.

V. Limit the aperture of the chimney.

VI. The fire may be kindled by a current of air brought from without, not using any of the air already warmed. If this theory, which is founded on true principles and reason, be compared with common practice, the errors will appear the disadvantages of which may be evaded.

VI Supply the combustion with air from without.

Experiment or trial.

III. I had a stove constructed to put the theory as fully in practice as possible, and have found all to answer according to theory.

A stove described in general terms.

The operations and effects are as follows, viz.

I. It applies the fire to warm the air as it enters the room, and admits a full and fresh supply, rendering the room moderately warm throughout.

II. It effectually prevents the cold air from pressing in at the chinks or crevices, but causes a small current to pass outwards.

III. It conveys the cold air out of the room first:—consequently,

IV. It is a complete ventilator rendering the room healthy.

V. The fire may be supplied in very cold weather by a current of air from without, that does not communicate with the warm air in the room.

VI. Warm air may be retained in the room any length of time, at pleasure; circulating through the stove, the coldest entering first to be warmed o [illegible]

VII. It will bake, roast, and boil, equally well with the common tin-plate stove, as it has a capacious oven.

VIII. In consequence of these philosophical improvements it requires not more than half the usual quantity of fuel*.

Ex. 4 Art of hulling rice.

Example IV. The art of hulling and cleaning rice.

Principle. The outer coat of the grain is sharp, and the surfaces if rubbed together, cut each other.

Step 1. The principles on which this art may be founded, will appear by taking a handful of rough rice, and rubbing it hard between the hands; the huls will be brushed off, and by continuing the operation, the sharp texture of the outside of the hull (which through a magnifying glass appears like a sharp fine file, and no doubt is designed by nature for the purpose) will cut off the inside hull; the chaff being blown out, will leave the rice perfectly clean, without breaking any of the grains.

II. What is the best plan in theory for effecting this†?

Plan for practice.

III. The disadvantages of the old process are known to those who have it to do.

Art of preventing ships from sinking.

Example 5. To save ships from sinking at sea.

Principles. why bodies float stated in a popular way.

Step 1. The principles on which ships float, are the difference of their specific gravities, from that of the water, bulk for bulk, sinking only to displace water equal in weight to the ship; therefore, they sink deeper in fresh than in salt water. If we can calculate the cubic feet a ship displaces when empty, it will shew her weight, and subtracting that from what she displaces when loaded, will shew the weight of her loaded. Each cubic foot of fresh water being 62·5lbs. if an empty rum hogshead weigh 62·5lbs. and measure 62 cubic feet, it will require 875·lb. to sink it. A vessel of iron, &c. filled with air, so large as to make its whole bulk lighter than so much water, will float; but if the air be let out and filled with water it will sink. Hence, we may conclude, that ships loaded with any thing that will float will not sink, if filled with water; but, if loaded with any thing specifically heavier than water, will sink as soon as filled.

II. This appears to be a true theory.—How is it to be put in practice, in case a ship springs a leak that gains on the pumps?

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* The discription will appear in a future number of our Journal.

† He describes a machine, which likewise deserves to be attended to, though less immediately connected with the industry of Great Britain. I shall consider it. W. N.

Practical result.

III. The mariner who understands well the above principles and theory, will be led to the following steps:

Throw the heaviest things overboard.

1st. To cast overboard such things as will not float, and carefully to reserve every thing that will float, for by them the ship may at last be buoyed up.

Empty the casks, and bung them well up.

2nd. Empty every cask or thing that can be made water tight, and put them in the hold, and fasten them down under water, filling the vacancies between them with billets of wood, even the spars and mast may be cut up for this purpose in desperate cases, which will fill the hold with air and light matter, and as soon as the water inside is level with that outside, no more will enter: if every hogshead buoy up 875lbs. they will be a great help to sustain the ship, (but care must be taken not to put the empty casks too low, which would overset the ship) and she will float, although half her bottom be torn off. Mariners for want of this knowledge often leave their ships too soon, taking to their boat, although the ship is much the safest, and does not sink for a long time after being abandoned; not considering, although the water gain on their pumps at first, they may be able to hold away with it, when arisen to a certain height in the hold; because the velocity with which it will enter, will be in proportion to the square root of the difference between the level of water inside and out; added to this, the fuller the ship, the easier the pumps will work; therefore, they ought not to be so soon discouraged.

Ships are safer than boats.

Pumps are easily worked in a water-logged ship; which may, therefore, be long kept afloat.

V.

Useful or Instructive Notions, respecting various objects. 1. *Multiplying of Copies of Writing.* 2. *Scintillation of the Stars.* 3. *Large Achromatic Lenses.*—W. N.

1. *Art of Copying, or of multiplying Copies.*

EVERY one is aware of the invaluable benefits which society has derived from the arts of printing, by moveable types, as well as by blocks and copper plates. But there are many cases, in which it would be of advantage to produce copies of writing, without requiring a stock of types or engraved plates; and the presses, or implements, by which the impression is made. A

Benefits of the art of printing.

saving, either in machinery, labour, or skill, is much to be desired. Under the present head, I have a few observations and facts to offer, relative to manuscript writing. The celebrated James Watt, about thirty years ago, obtained a patent for a copying machine, for making copies of the description, known by the name of counterproofs. His apparatus, consisting of a portable rolling press, a receptacle for keeping very thin unsized paper in a due state of wetness, and a peculiar ink more mucilaginous and less speedy in drying than common writing ink, is at present in general use, particularly in merchants' counting houses. In a former Journal it was remarked, that sugar or treacle, added to ink, gives it the disposition to come off upon wet paper, and that if the paper be well soaked, so as not to shine and yet to be considerably transparent, a very light pressure, such as that of a warmed flat iron, would produce the copy.

James Watt's copying machine,

It is to be regretted, that this ingenious application shoul require as much apparatus and skill as it does; though its value is undoubtedly very great. The following process is less neat, but may be practised wherever a round ruler and gauze paper, or blotting paper, can be had. I have availed myself of it on a journey; in which it first occurred to me as an expedient for copying letters.

requires preparation and apparatus.

The process.—Roll a piece of guaze paper upon a small sound ruler, and place the ruler, thus covered, upon the sheet of paper intended to be written upon, in such a manner as that the ruler shall be just above, and parallel to the intended first line, and the outer edge of the gauze paper on the same side as the upper edge of the paper. Then write the first line, and immediately upon concluding the same, roll the ruler just upon it; and the gauze paper will receive a print of that line. Return the ruler to its first position, write a second line, and take a print of that as before.—And in this manner the whole letter may be copied while writing. I found a little awkwardness at first, in bringing myself into the habit of this manipulation; which requires the writer to recollect, at the end of every line, that he is to apply the gauze paper; but this was soon overcome. And it may also be observed, that for a very light hand, which dries quickly, it would probably be needful to apply the ruler at shorter intervals. My hand writing, which is neither heavy

Another process which can be practised in all situations.

heavy nor light, admitted of the operation being performed, as before directed, but I could not defer it to any second line.

Art of making copies by black tracing paper.

Another artist, of the name of Wedgewood, has, within a few years past, offered to the public, under sanction of Letters Patent, the engraver's method of tracing, by means of a piece of paper blacked with a pigment, (commonly lamp-black) applied by means of fat or a slowly drying oil. If such paper, which is sold at the shops, by the name of black tracing paper, be laid upon a leaf of common paper, and another leaf be laid upon that, the whole being disposed upon a firm flat table or plate of wood, or metal, or glass, and any writing be made with a small rounded steel or glass point, two copies will, by the same operation, be produced; viz. a reverse copy on the upper white paper, and a direct copy on the lower; the latter of which is sufficiently durable to be sent away to a correspondent, and the former will be very legible, as a direct copy, if the paper be thin.

A method of speedily writing upon, and printing from a metallic plate.

Dr. Franklin mentioned to the Abbé Rochon* a method of rapidly engraving or marking plates, for multiplying copies. He wrote with gummed ink, upon a surface of hard stone or ron, and powdered his writing with sand, or emery, or cast ron dust; and when dry, he applied another plate of soft wood, or pewter, or copper, upon the surface, and forced the gritty matter into this last by the action of a press. This last erved, in the usual method of copper plate printing, to give a very great number of copies, not neat or beautiful, but sufficiently legible.

Another by etching and making counter-proofs.

The Abbé Rochon proposes, as a better method, to write with a steel point upon a copper plate ready varnished, and etch he face by aqua fortis. Reversed prints being taken from this etching, he piles these, while wet, along with other damped paper, and passes the whole through a press, which gives an equal number of counter proofs not reversed.

Improvements suggested.

Both the last mentioned methods may be of use in armies and under other circumstances: but both suppose extensive means and apparatus, and only dispense with the engraver's skill. Perhaps it would be an addition to Rochon's method, that the

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[*] Recueil de Memoires, &c. from M. L'Abbé Rochon, octavo, Paris, 1783, p. 343.

etching should be omitted, and the writing made upon soft metal with a sharp point leaving the bur on. Such a plate would afford many impressions.

It would be a great improvement upon Watt's method, if the Counter-proofs could be taken upon dry paper. The tracing paper of Wedgwood and the engravers soon loses its colour, and it will not keep long. It soon becomes too dry to give off its colour.

2. *Scintillation of the Stars.*

Twinkling of the stars ascribed to the air.

Many speculations have been offered to account for and explain that apparently irregular and agitated emission of light, from the fixed Stars, which has been called scintillation or twinkling. From its marked appearance at low attitudes, and almost total absence at higher, it has been commonly ascribed to the interposed atmosphere; which, by the changeable densities of its parts, and the interposition of opakė particles, is imagined to produce variations in the quantities, colours, and directions of the light before it arrives at the eye. In proof of this doctrine it has been farther noted, that the stars do not scintillate in a telescope. Undoubtedly the effect is still clouded with uncertainty. An observation I made upon the Dog Star (Sirius) in the autumn of 1807 may be considered as affording a few facts more in addition to those we already possess.

The stars do not twinkle in a telescope,

It is not true that the stars have no scintillation in a telescope. It may be strikingly observed by putting the instrument out of adjustment. In this case the circular disc of light, has a kind of vaccillation, as if a number of discs were continually flashing before each other: the illumination seemed to come on at different sides, and these discs also differ in colour.

but give coloured rays in succession.

Blue, steel blue, pea-green, bright copper, red and white, are among the most usual colours; but the rapidity of succession does not allow the sense to determine whether these colours may be more or less cotemporaneous, or completely and distinctly succeeding each other. To determine this point, I

An experiment on the coloured rays of the Dog Star.

took an achromatic glass of Ramsden's, magnifying 24 times, and directed it to the star—the object end being supported in a notch in a steady bar connected with the wall, and the eye end, upon an adjustable piece which was likewise capable of being set very steadily. But upon this I rested my left hand, between the finger and thumb of which I held the eye end of the glass. In this situation, the glass being truly adjusted to distinct vision, I could observe the star, and by gently and rapidly striking the tube with the fingers of the other hand, I caused the image of the star to dance in the field of view, and describe the same kind of luminous line as is seen when a lighted coal is whirled about. The star was thus made to describe by each blow a curve returning into itself; but so contorted and irregular that no two successive curves were coincident with each other. The strokes were about ten in a second of time, and the curves were beautifully and distinctly tinged with different colours in their successive parts thro' different lengths: but it seemed at a medium that each of these vivid colours might occupy about one-third part or less of the whole curve, and upon my recollection those most predominant were greenish blue, steel blue, and maroon or an intense copper colour. The light from Sirius therefore as it arrived at the eye was by extremely sudden variations distinctly changed in its colour, at least thirty times in one second. No theory deducible from the known properties of the atmosphere, as an interposed medium, has yet presented itself to my mind, in a shape worthy of notice.

The rays were beautifully distinct and vivid.

and varied thirty times in a second.

In the collection last quoted of Rochon, p. 380, he observes, that the scintillation of the fixed stars is an obstacle to measuring their diameters, and that when the light of Sirius was refracted into colours by a prism, it had no scintillation across the spectrum. As far as may relate to the apparent diameters of the fixed stars, the observations of Herschel do not seem to support the deduction of Rochon; but his fact appears to correspond with mine.

Correspondent fact with a prism.

3. Advantage of upsetting or pressing in the borders of plates of flint glass to make the concave lens in achromatic combinations.

The same Abbé Rochon p. 372, remarks that the triple object lenses of Dollond of 3½ inches aperture, produce an effect equal to that which it seems ought to be obtained from the lenses of 30 or 40 feet, made by Campani. But that in making achromatic lenses of longer focus, the plates of glass being blown, are too thin to be worked without bending and spoiling the figures. All the cast glass he tried was found to be

Large achromatic lenses cannot be made for want of thick flint glass.

Blown glass is superior to cast.

be more unequal in its quality, in different parts of the same plate, than the blown glass. It would not be difficult to explain this from the circumstances of the making; but the principal object of the present notice is, to mention that he succeeded in making a thick lens out of plate one quarter of an inch in thickness, by softening the glass by heat upon an earthen mould of the proper curvature, and upsetting or pressing the borders inwards, (taking care to avoid folds or wrinkles,) till the edge was an inch thick, and the diameter five inches. He then surrounded the glass by a metallic ring of six inches diameter, and three quarters of an inch deep. Within this ring he again heated the glass, upon which he previously placed an upper convex earthen mould. The glass thus obtained appeared very good, and when ground and polished, enabled him to make a triple object glass of seven feet focus, producing, as he says, a much greater effect than the glasses of Dollond, but without admitting of a proportionate aperture. For the lenses of that celebrated artist bore an aperture of 42 lines, and his lenses would not admit of more than 4 inches or 48 lines; which, however, adds more than one third to the whole quantity of light. From the great care in working, he did not think that the external parts of the lens were defective on account of the figure. The defect arose most probably from the flexure and contortion of the grain of the glass in pressing in. For an ingenious philosophical artist has assured me, that there is great difference in lenses and prisms made of the clearest plate glass; accordingly as the line of vision is directed at right angles to the natural plane, or more obliquely or coincident with it, the latter being in general good for nothing. Whence, and from other facts, he inferred that the layers of glass plates differ considerably in their densities.

Blown plate glass may be worked up thicker.

A lens, made in this manner, was excellent;

but the borders defective, probably from the glass and not the figure.

VI.

An Account of some Experiments on the Congelation of Mercury, by means of Ether. By ALEXANDER MARCET, M. D. F.R.S.

To Mr. Nicholson.

SIR,

Account of Mr. Leslie's method of freezing.

MR. Leslie's new and ingenious mode of illustrating the well known fact of the production of cold by evaporation, by actually freezing water, in consequence of a rapid process of vaporization from the water itself, has already become a familar experiment. Water is placed over an open vessel, containing sulphuric acid, and the whole being inclosed within the receiver of an air pump, the water cools as the exhaustion proceeds, and is ultimately converted into ice. I have learnt also, that Mr. Leslie has succeeded in freezing mercury by a similar process; that is, by investing the bulb of a mercurial thermometer with a thin coat of ice, and exposing this to the joint effect of exhaustion and of sulphuric acid.

Mercury frozen by evaporation of ether.

After trying to repeat the last of these experiments, (an attempt in which I did not succeed) I effected the congelation of mercury with great facility and quickness, simply by substituting the evaporation of ether, instead of that of water, in the process in question. I am not aware of having been anticipated in this experiment; if I have, you will oblige me by taking no notice of this letter; but, in the contrary case, I shall thank you to give it a place in your Journal.

Method of making the experiment.

The mode in which the experiment is made is this: a conical receiver, open at the top, is placed on the plate of the air pump, and a mercurial thermometer is suspended within the receiver through the aperture. This is done, like some of the well known pneumatic experiments, by means of a brass plate perforated in its centre, and fitting the receiver air tight when laid upon its open neck. The thermometer passes through this plate to which it is carefully fitted by a leather adjustment, or simply by cork, secured with sealing wax; and it is so graduated, that when its bulb is sunk a few inches within the receiver, the stem rises externally through the plate, above which the scale begins. The bulb is then wrapped up in a little cotton wool, or what is better, in a little bag of fine fleecy hosiery; and after being dipped into ether, the apparatus is quickly laid over the receiver, which is exhausted as rapidly as possible. In two or three minutes the temperature sinks to about 45 below 0, at which moment the quicksilver in the stem suddenly descends with great rapidity, (in consequence of the remarkable contraction which the mercury in the bulb undergoes in congealing) to a distance corresponding to between 300 and 400 degrees. This, however, seldom happens to that extent, because the descent of the mercury is often impeded by the freezing of the column itself at the entrance of the bulb, before the congelation within the bulb is completed.

The facts particularly stated.

If it be desired to exhibit the mercury in its solid state, common tubes may be used, which should be broken instantly after being removed from the pump. I have frozen in this way bulbs of an elongated shape, about an inch in length, and near an inch in diameter. The pump I have used for these experiments is one of a small size;* the gage of which stands at about a quarter of an inch, when the exhaustion is pushed to its utmost extent. I have occasionally succeeded in this experiment, when the temperature of the room, as well as that of the ether, was about 50°; but the certainty of success is much increased by operating in a room, the temperature of which does not exceed 40°, and by previously reducing the temperature of the ether. I have been in the habit, in making this experiment, of inclosing sulphuric acid within the receiver, as in Mr. Leslie's process, as it has appeared to me to promote the evaporation of the ether, and the production of cold; but the experiment has also succeeded without the assistance of sulphuric acid.

Variation of the experiment.

The same experiment may be varied by first dipping the bulb of the thermometer, surrounded with cotton wool or flannel, into water, and after freezing this by means of the pump, pouring a few drops of ether upon the frozen bulb, and exposing it again to the effect of exhaustion. This plan has sometimes succeeded when circumstances were not sufficiently favourable for the success of the other.

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* Made by Mr. Bate, instrument maker in the Poultry.

Water frozen, by applying the air pump to Dr. Wollaston's instrument.

I have applied a method, similar to those just described, to the freezing of water, by means of the ingenious instrument imagined by Dr. Wollaston*, to which he has given the name of chryophorus. This instrument consists in a tube, terminated at each extremity by a ball, like the common pulse glass, one of these being full of water, and both the balls and tubes being completely exhausted of air. By plunging the empty ball into a mixture of salt and snow, the water in the other ball, though at some inches, or even some feet distance from the cold mixture, is frozen in a few minutes. But by a process, similar to that I have just described, for the congelation of mercury, the same may be effected without any cooling mixture in less than one minute, and with a pump of very moderate power. I may take this opportunity of mentioning, that having constructed an aparatus of this kind, with a thermometer within it, I observed that the temperature of the water sunk to 20° ; and, in one instance, even two or three degrees lower before it froze, which I at first ascribed to the water being deprived of its air by previous boiling ; but the same circumstance not having uniformly taken place, when the shape and size of the apparatus, and the quickness of the process, were varied, I am now inclined to ascribe it to other causes.

I have the honour to be, &c. &c. &c,

ALEXANDER MARCET.

Russell Square, 22nd Jan. 1813.

VII.

Observations upon the best state in which it is advisable to bring the British Merino Wools to market. By EDWARD SHEPPARD, Esq. of Uley, in Gloucestershire.

Introduction.

MR. SHEPPARD has made his title good to that fame which attends the patriotic and well-directed exertions of so many of our country gentlemen, in improving our valuable stock of first materials. Wool has, for centuries, been considered as one of the first, and Mr. S. has claimed and received the Gold Medal of the Society of Arts, for having produced from his flocks of 1929 Merino and Merino Ryland, the whole bred and kept by him, 7749 lbs. of wool in the year 1812. He has communicated the following observations to the society.

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* This apparatus was described a few weeks ago, by Dr. Wollaston, in a paper which was read before the Royal Society, an abstract of which was published in the 1st number of Dr. Thomson's Annals of Philosophy.

The Author's experience in the growth and manufacture of wool.

Having had the experience of more than ten years, both in the growth and manufacture of British Merino wools, which, by the constant use of the Spanish rams that came into his Majesty's possession during that period, I have brought to very great perfection; I take this method of making public the result of my observations, as to the mode most profitable for the grower and manufacturer, to prepare the Merino wools for the market; as considerable difference of opinion and practice prevail on the subject.

The superior softness of the Saxon and Anglo-Merino wool arises from their being kept in their grease.

I had the honour, in the year 1806, to present a memoir to the Board of Agriculture, in a successful claim I made, for the Gold Medal given for the greatest quantity of fine wool, grown within the year. I therein stated my opinion, that the principal cause of the superior and characteristic softness of the Saxon and Anglo-Merino wools, was, their remaining in their native grease, without its being expunged in the extreme degree practised in Spain. Excepting the moderate washing that Saxon and British wools receive on the sheeps' back before shearing, they continue in their grease till they are worked up by the manufacturer; while the wools in Spain, as soon as shorn, are thoroughly scowered, by an injudicious process, and then exposed for days to a burning sun, in which brittle and hard state they are so closely packed up, that they come out of their bags here, almost as much pressed and hard as hops, wholly deprived of that unctuous preservative, which I conceive to be necessary to the soft feel of wool.

The wools of Spain are harsh because too much scowered,

and would most probably be soft if better managed.

It has been thought by some, that Saxon and Anglo-Merino wools have a softness peculiar to themselves, and different from the Spanish, their parent stock, obtained from their cross with another and coarser woollen sheep. I am, however, very much disposed to attribute the quality here spoken of, to the better management of the wools in this country. Unfortunately, we have no opportunities of discovering what Spanish wool would be preserved in the grease; as the mode of laying on the duties at Burgos, by the pound, prevents the grower or merchant exporting it in that condition. Otherwise, I am much inclined to think the same softness would be found in the pure parent fleece, as in the spurious offspring. From the small experience afforded by the ill-conditioned fleeces lately imported with the sheep from Spain, I am very much confirmed in my opinion.

Lambs' wool of Spain is less cleansed, and is softer.

Lambs' wool, not being so completely washed from its grease in Spain as sheep's wool, comes very near to the softness of the Saxon and British lamb's wool. As a proof of their possessing an extra quantity of grease, they are much sooner liable to breed the worm than Spanish sheep's wool. I have often proved, in the manufacture of wool, that where it has been long saturated with oil, artificially, the fibre has been lubricated with it, and the cloth very superior in feel and softness.

Vell wool is very harsh because cleared of all grease,

It has long been known to manufacturers and wool-staplers, that the wool of dead sheep, or Vell-wool, as it is called, is very harsh, and quite unlike the same wool shorn from the sheeps' back, occasioned by its being disengaged from the skin, by the fell-monger, by the action of lime, which entirely dries up and destroys the oily particles. May it not, in some measure, arise from the cause, that wool from sheep used to calcareous or silicious soils, is of a harsher description; as those from the Sussex, or Wiltshire downs, when compared with the fleeces grown on the argillaceous lands of Hereford and Shropshire? The absorption of the native grease, by the frequent contact of the sheeps' coat with the soil, and the dust from it, may help to remove that great preservative of softness, and leave the fibre exposed, unprotected by moisture, to the action both of the sun and rain, which, in those exposed situations, would act with double power.

and this may be the cause of harsh wool on chalky soils.

Inferences: that the sheep should be protected.

From the above theory I would wish to deduce a few inferences, which may be of service in the growth and management of British fine wools. In the first place, I am satisfied that nothing can so much tend to preserve this necessary state of washed,

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and that the fleeces should be kept in the grease as long as may conveniently be:

My opinion, as to the bes
for the market, is, that whe
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The wool may

Clean wool weighs only half of native wool in the grease.

The waste on British Merino wool, which has never *been washed on the sheep's back*, is rather more than one half, or about 10lbs. in 20, reckoning to its clean picked state. The same wool, *when washed on the sheep's back*, loses with the manufacturer about one third, or from 6 to 7lbs. in 20, which is about the average of the waste of Saxon wool. Whereas, the best imported Spanish wools will not waste more than half that amount: viz. from 3 to 4lbs. in 20. It is obvious, that a proportionate difference must be made in price, for the different conditions in which British Merino wools are produced; the manufacturer will be better able to estimate the probable waste of the wool that has been washed on the sheeps' back, as there is so much dirt, sand, and filth, generally with the wool in its genuine, unwashed state, that the waste must be always uncertain. I think, therefore, that wool washed on the sheep's back will be the most merchantable.

Lamb's fleece should be cut early, because it is deteriorated by exposure.

I would also remark on the most preferable mode of managing the lamb's fleece, which I should recommend cutting, in preference to remaining on the lamb, till he becomes a yearling, as practised by many. The external part of the hog's fleece, which was the original lambs-wool, suffers most materially from the inclemency of the weather and the winter. In its state of lambs-wool it is beautifully soft, but being afterwards protruded from the new coat, it is in that condition exposed to the snows, winds, and rains of the winter, by which it becomes entirely deprived of its grease, and as coarse as the wool of our common country sheep. The deterioration of this exposed part of the fleece, in one season only, fully proves what effect climate and weather have on the fibre of wool; it is therefore certainly desirable to shear the lambs, as in Spain; and although the covering may be more complete for the young sheep against the winter with the lambs coat on, yet the being rid of the incumbrance of a wet draggled fleece, in deep soils and bad weather, is of great advantage to the young and tender sheep.

EDWARD SHEPPARD.

Uley, Gloucestershire, March 5, 1812.

General

VIII.

General Results of Beccaria's Observations upon the Electricity of the Atmosphere during serene weather; together with those of Romayne and Henley. Abstracted by a Correspondent. (R. B)

To William Nicholson, Esq.

SIR,

AFTER the systematic arrangements of clouds by M. Luke Howard, and his speculations upon their formation and disappearance, which I consider as having greatly enlarged and regulated our knowledge and means of making atmospheric researches,—and particularly from the probability that the disposition, and even the notions of clouds, may be in a great measure referable to the ordinary phenomena of electrified bodies, I have thought it would be of service to the inquiries of other observers, to send you an abstract which I made for myself, of the facts and remarks of these very diligent and faithful observers; whose works, from their extent, their dispension, and even their date, though well esteemed by philosophers, are at present less likely to be referred to. At all events, I submit to your judgment, and am, without farther preface,

Sir,
Your most obliged reader,
R. B.

Value of Beccaria's observations.

THE numerous and important observations of Father Giambatista Beccaria, on Atmospherical Electricity, render his conclusions on this subject highly estimable. His treatise annexed to the English translation of his Artificial Electricity deserves to be consulted. At present, I shall do little more than give his propositions or general results.

Apparatus, a long insulated wire, exposed to the atmosphere.

The apparatus by which those results were obtained, was settled on the pleasant hill of Garzegna, in the neighbourhood of Mondovi; from which the whole compass of the Alps, as well as the whole plain of Piedmont is easily discovered. It consisted of an iron wire one hundred and thirty two French feet long, extending from a stack of chimnies, over which it was raised by a long pole to the top of a cherry tree. Its extremities were insulated and defended by a small umbrella of tin, covered beneath with sealing wax. From this wire, another was introduced into a room through a pane of glass. It was found

Pith balls were connected with the wire.

The electricity is almost constantly positive in clear weather.

1. That the electricity, during serene weather, in its ordinary or mean state, causes two balls of pith of elder one line in diameter, to diverge six lines from a small plate of metal placed between them. The balls were suspended by very fine treads, sixteen lines long. 2. In the state of its greatest intensity the divergence of the balls is fifteen, twenty, or more degrees from the metal. 3. In its weakest state the balls move towards a conductor at a very small distance. 4. The electricity is sometimes so slow in its accumulation as to require one minute to become again sensible, after having been taken off by touching the wire; but at other times it became again sensible in the time of one second. 5. That it is always of the positive kind, excepting in some very rare instances, when the contrary happens, in consequence of the wind blowing from some other part of the sky which is not serene. The instances related by Beccaria are very curious.

He used an hygrometer of flaxen thread and another of oat beard.

Father Beccaria used an hygrometer consisting of a string of thirty-two flaxen threads twisted together to the thickness of two thirds of a line. It was twelve feet long, and the lower part passed round a pully which carried an index. The stretch-weight was two pounds. Such an hygrometer commonly served him a year, and he distinguished smaller mutations than it was capable of shewing by means of another hygrometer made of a twisted rye-stalk.

The electricity in clear weather is proportioned to the humidity.

6. During clear weather the moisture in the air is the constant conductor of the atmospheric electricity; and this electricity, is proportioned to the quantity of that moisture which surrounds the wire, except such moisture lessens the insulation both of the wire and of the atmosphere.

Beccaria observes, that he does not here pretend to point the cause or principle which produces the electricity, but only to ascertain the medium in which it is inherent, and to the quantity of which it is generally proportioned.

7. The

It is always [illegible] the [illegible] of [illegible] er, [illegible] re [illegible] produced as the evap is more rapid.

7. The electricity that takes place when the weather clears up is always positive. When the air takes up moisture very rapidly the intensity of the electric state of the wire, as well as its quickness in becoming again sensible when destroyed are great; but the latter diminishes as the weather becomes dryer. It sometimes happens that the electricity thus caused continues a long time in its state of intensity, and begins afresh after being interrupted. Beccaria thinks these effects are owing to electricity being brought from great distances by the wind.

Particular observations of clouds, their figures and changes, and the concomitant electricity.

8. If the sky becomes clouded over the place of observation, and only an high cloud is formed without any secondary clouds under it, and the cloud itself be not part of a cloud that drops rain elsewhere, then the electricity of the wire is either positive or null. But if the clouds resemble locks of wool moving to and from each other; or if the general cloud is forming very high and is stretched downwards like descending smoke, then a frequent positive electricity commonly takes place, which is more or less strong in proportion to the quickness with which the cloud is forming, and foretels the quantity and suddenness of the rain or snow which follows. 2. When a rare, even, and extensive cloud is forming, which darkens the colour of the sky, and renders it grey, positive electricity, very intense and speedily recovering its intensity when taken off, is produced; which state diminishes and even fails as the gathering of the cloud slackens; but on the contrary, if the cloud continues to increase gradually by the accession of smaller clouds, resembling locks of wool which are continually joining and separating, the positive electricity usually continues. 3. Low and thick fogs (especially when they rise into a superior air considerably free from moisture) carry up to the wire electricity which gives frequent small sparks, and the balls diverge between 20° and 30°. If the fog seems stationary and continues to environ the wire, the electric signs soon disappear; if it continues to rise and another cloud of fog succeeds, the wire is again electrified, though less than before. Sky rockets sent through such thick low and continued fogs have often afforded our celebrated observer signs of electricity by means of a string affixed to them. He never, however, observed in any of the above circumstances, any signs of negative electricity except once by a sky rocket sent through a fog, in which he saw the

Fogs give positive signs.

Rockets made use of

star of electric light denoting negative electricity, but thinks that he might have mistaken its figure.

Romayne and Henley.

As Father Beccaria in this place mentions his two fellow labourers, Romayne and Henley, I shall here take occasion to notice their observations, and then resume my subject.

M. Romayne's apparatus, an electrometer at the end of a pole.

Mr. Romayne* made his experiments between the year 1761 and 1772. He held an electrometer, consisting of two cork balls, suspended by threads six or seven inches long out of a garret window, by means of a pole five feet long; and to these, when elecrified, applied excited glass, or sealing wax, by the help of another pole, and by that means determined the kind of electricity.

He found positive electricity.

He found the air at a proper distance from buildings, ships' masts, &c. to be very sensibly electrified during winter, in foggy or in frosty weather; less so in mists, and still less in calm and cloudy weather. But in summer he never observed any electricity, except during a fog in the cool of the evening, or at night. He never found any electricity during the time of an aurora borealis, unless a fog happened at the same time; excepting once, and then it was weakly positive.

He always found the electricity of the air to be of the positive kind; excepting once only, during a fog, on an uncommonly warm day in winter.

When a fog became very thick, he observed that the cork balls came nearer to each other, but opened again on its recovering its former state; and he also found, that rain during a fog produced the same effect, which ceased as soon as the rain was over.

Smell of fogs and of the electric spark.

Mr. Romayne also observed that the smell of fogs, and frequently of the common air, resembles that of an excited tube. He observes, that when the density of fogs floating near the earth increases considerably, the balls always approach; but that the reverse generally hapens when the fogs are high in the air. He once saw a struggle between breezes from N. W. and S. E. at the same time in which the one seemed sometimes to prevail and afterwards the other. The contention was preceded by a smoky haziness, like a fog, which occasioned the balls to diverge; as the haziness thickened they separated more, and the repelling power was augmented in proportion as the drops increased.

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* Phil. Trans. Vol. LXII. p. 137.

On this occasion, M. Romayne was the first who made an elegant experiment, to shew, that the diminution of surface increases the intensity of electricity in bodies. He found, by repeated trials, that a piece of flannel, silk, &c. excited and suddenly twisted, not only struck at a greater distance than before, but sometimes emitted parcels of fire into the air. And from this he infers, that the electricty of vapour, when not in contact with the earth, ought to increase by condensation. This is still farther confirmed by the experiments of Volta and of Bennet, on the electricity of vapour*.

Sudden changes of electricity, which might be explained by correspondent observations.

At other times, M. Romayne made use of a tapering tube of tin, twenty feet long, and ending in a point, insulated, and projecting upwards out of a window. He took notice of that uncertainty and frequent change in the electricity of clouds, which was before remarked by Dr. Franklin and others; and, after several ingenious observations, he expresses his wish, that two or more persons, at a sufficient distance, would correspond by signals, indicating positive electricity by a red flag, and negative by a blue; as it is highly probable that much more satisfactory knowledge would be thus obtained, respecting the electricity of the clouds, thunder, &c. than any single observer could acquire.

The observations of Mr. Henley † tend to corroborate those of Mr. Romayne, but do not lead to any further conclusions.

General facts.

I now proceed in the enumeration of general facts, or the propositions of Beccaria.

Single clouds diminish the intensity in the wire. Separate masses increase it.

9. In clear weather, when a low cloud, considerably distant from any other, happens to pass slowly over the wire, the positive electricity is usually much diminished, but is not rendered negative; and, when the cloud is gone, it returns to its former state. But, if numbers of whitish clouds, resembling locks of wool, continually uniting and separating, remain over the wire, so as to form a considerable extent, the positive electricity

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* And more fully by the condenser and well-known experiments, made with Bennet's gold-leaf electrometer.

† Ph. Trans. vol. 62. p. 145, and vol. 64. p. 422.

city commonly increases. The electricity never becomes negative in either of the above cases.

Father Beccaria, in his experiments on the electrified air of a room, found that the electricity is proportional to, and therefore most probably resides in the vapours floating therein. The same conclusion may, therefore, as he observes, be naturally applied to the atmospherical electricity, which is not sufficient in general to produce electric figures, in electrometors which are not insulated. The two last propositions, 8 and 9, relate to such phenomena as take place when the weather either becomes overcast or clears up. The following relates to the effects of vapour or moisture, as shewn by the hygrometer.

The artificial electricity in a room is proportioned to its moisture.

10. In the morning, if the hygrometer indicates a great degree of dryness, very little difference from that of the preceding day, then even before sun-rise an electricity takes place, causing junction, adhesion, or divergence, of the ball; and its intensity is greater the drier the air, and the less that dryness differs from that of the preceding day. But if no such great dryness obtains, no perceptible electricity takes place, till sunrise, or a short time after.

In very dry mornings electricity * is shewn; but if the air be not very dry, the electricity appears after sun rise.

11. The electricity of the air gradually increases as the sun rises higher. The gradual increase begins sooner, according as the hygrometer continues after sun-rise to indicate a higher degree of dryness, and as such dryness more speedily increases. This increase, both of intensity and speedy recovery, when taken off, last in serene days, when the wind is not violent, till the sun draws near its setting, provided the hygrometer keeps near the highest degree it has reached. But when the sun is near setting, and in proportion as the hygrometer retreats, the intensity of the daily electricity is diminished, at the same time that the quickness with which it is revived in the apparatus, when taken off, becomes greater.

The electricity increases as the sun rises higher, &c.

12. Though the hygrometer may indicate equal degrees of dryness in the middle of the day, on different days, yet the time in which the apparatus recovers its electricity on those days is less, the greater the increase of heat; and when the heat is greater, the electricity arises later in the morning, and fails sooner in the evening

Difference when hemoisture and heat are differnt.

13. The friction of winds against the surface of the earth is not the cause of atmospheric electricity. Impetuous winds dimi-

Winds do not produce electricity by friction.

nish the intensity of the electricity of clear weather. And if they be damp they diminish its intensity, by rendering the insulation both of the atmosphere and of the apparatus more imperfect.

Other facts and observations on the friction of winds.

Father Beccaria made many experiments to discover whether the friction of air against conducting bodies produced electricity. He used the bellows, and also turned fans of gilt pasteboard very swiftly round on an insulated axis, but obtained no electricity either in damp or dry weather. He had before observed, that air produces electric signs, when it strikes their glass*. He found also that the umbrella, with an insulating handle, which the French call paratonneres, never exhibited the least electricity when held obliquely to the wind. To these I may add, that the very sensible electrometer, of Bennet, does not become electrified by blowing pure air upon it†. The proposition of Beccaria does not, however, rest upon electrical experiment, but is likewise supported by a variety of actual observations on the state of the atmosphere. And though these cannot be transcribed, on account of their length, yet I am unwilling to pass over in silence his very cogent remark, that if the electricity in any degree arose from the friction of winds against the ground, it would be found the greatest near the surface of the earth, but the contrary is the fact.

Night electricity in cold dry air,

XIV. In cold weather, if the sky be clear, the wind not violent, and the air considerably dry, an electricity of considerable intensity arises after sunset, as soon as the dew begins to fall. The quickness with which the apparatus recovers its electricity after being touched, is greater than during the diurnal electricity, and it disappears very slowly.

and also in temperate or warm weather.

XV. In temperate or warm weather, and in the same circumstances of wind and moisture, an electricity perfectly similar to the above takes place as soon as the sun has set; but its intensity is not so constant, it begins with more quickness, rises to a state of more speedily recovering its intensity after being touched, and ends sooner.

Moisture affects the insulation.

XVI. When the air in the above circumstances is less dry, the electricity is less intense, by reason of the insulation being rendered more imperfect, but its quickness in recovering its intensity after contact, is greater, as the quantity of dew is greater.

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* See p. 363, vol. I. on Artificial Electricity. ʃ776.

† Ph. Trans. vol. LXXVII. p. 30.

Dew acts nearly like rain.

XVII. The electricity of dew seems to be, in proportion to its quantity, in the same manner as the electricity of rain depends on its quantity; and the peculiar manner or circumstances which attend the falling of the dew, influences the electricity in the same way, as does the peculiar manner in which rain takes place.

Succession of similar phenomena.

XVIII. As rain, showers, aurora borealis, zodiacal light, have a tendency to begin afresh for several successive days, with the same characteristic accidents, so the electricity of dew seems to have, as it were, an inclination to appear for several evenings successively, with like characters.

An experiment of artificial dew in a room artificially electrified.

After these propositions relating to the dew, father Beccaria adds the following: let the air, in a closed room, be electrified, that is to say, the moisture and other vapours diffused in it; let a bottle filled with water, colder than the air of the room, and insulated on a stove of glass, be raised pretty high in the room, and the insulation be carefully preserved. Then the electric signs that will arise in two threads suspended to the bottle, will exactly represent the electricity of dew, for they will exhibit the different manners after which this electricity takes place, according as the electrified vapours in the room are more or less rare; as the difference between the heat of the bottle, and of the air in the room, is more or less; and as the insulation of the bottle is more or less accurate.

This excellent and most industrious philosopher, after reciting various facts respecting the electricity of dew, concludes with the following summary observations:

The diurnal electricity resembles that of a fog, and the night electricity that of a shower.

The diurnal electricity resembles the electricity of a very rare fog, which rises, becomes dilated, and by that means, continually renders the insulation more perfect. The nocturnal electricity resembles that of a very rare and subtle rain, which descends, becomes condensed, and continually renders the insulation less perfect, whenever the diurnal electricity is more constant. But the nocturnal electricity frequently fails, and only attains its greatest intensity when the increase of that moisture, which is the conductor of it, happens to take place without injuring the insulation.

Notice

IX.

Notice of an Adventurer to the Interior of Africa.

Interior of Africa.

IT was some time since mentioned, that a German, of the name of Roentgen, had been making preparations to prosecute the same objects of discovery that excited the ardour of the celebrated, though unfortunate, Park; and, penetrating into the central regions of Africa, to reach, if possible, the city of Tombuctoo, which has never yet been explored by any European traveller. The following article on this subject has appeared in a German journal of the 8th of October, quoted in the General Chronicle.

"There has been lately published, at Neuwied, an interesting letter from the traveller Roentgen to his brother. It reached him through Professor Hagen, who received it from Mr. Nunemann, of London. Roentgen, it appears, after visiting Paris, Vienna, and London, had repaired to Mogadore, where he resided a considerable time; and the letter in question, dated the 21st of July, 1811, was written on the bank of the river Teulifft, at the moment of his departure for the interior of Africa." The following is some of the most interesting information it contains:—

'During my residence at Mogadore, I was engaged day and night in studying the Arabic; and I have succeeded in making myself to be understood by the natives of the country. I will avail myself of that knowledge of the country, and of the manners of the people, which I have acquired, in order to travel to Tombuctoo. I would not act with so much boldness, were I not convinced, that providence has destined me to make the discovery of the interior of Africa. My good stars have furnished me with a companion in my travels, than whom I could not have wished for a better. He is a German, who, when only twelve years old, quitted his paternal roof, having an irresistible inclination for roaming; he has never since lived six months on the same spot, and is now thirty-eight years of age. He knows all the European languages, the Sclavonic excepted. Fourteen years ago, when destitute of money or protection, he was impressed by the English for a sailor, in an island of the Mediterranean, Interior of Africa.

Mediterranean, where he happened to be; he was inhumanly treated by them, and reduced almost to despair. His ship anchored before Tetuan, for the purpose of watering; and there, having struck an English officer who had used him ill, in order to avoid punishment, he escaped, and became a Mussulman at Tetuan. Since then, he has traversed the Barbary states in all directions, and has lately returned from a pilgrimage to Mecca. He has lived at Jamba, in Africa, as a coffee-house keeper, and at Janol, as a physician. At Constantinople, he has superintened the gardens of a Pacha. I got acquainted with him at a merchant's in Mogadore, who had hired him as a gardener. I have taken him into my service, and I treat him rather as a friend than as a domestic; the benefits which I shall derive from his experience are immense.'

'About a month ago, I travelled with a caravan of merchants to Morocco, where I procured valuable information respecting the communications with the interior of Africa.

'It is impossible to convey an idea of the violent hatred which animates the Moors against Christians. Even at Mogadore, I could hardly go abroad without being overwhelmed with insults. I was obliged, in order to view the city of Morocco, to get an escort of four soldiers, who, by orders of the government, were to keep back the populace. Even then I was often assailed by stones, one of which hit me so severe a blow on the forehead, that for some time I thought myself dangerously wounded. This hatred of the Moors arises in a great degree from our dress.

'I saw, at Morocco, preparations for the setting out of a caravan, which was to reach Tombuctoo by Tafilet and Tunt. I immediately formed a resolution to join this caravan, and I returned to Mogadore. My companion was delighted with the plan, which I did not communicate to any one else, but to one Christian. I caused it to be reported at Mogadore, that, disgusted with the bad treatment I had received at Morocco, I meant to repair to Tangier, and from thence embark for Gibraltar. This pretended project furnished us with a pretext for purchasing a mule, and every other necessary for my journey. I secretly procured some Moorish garments. Having finished mypreparations, I invited some Christians at Mogadore to a party of pleasure on a mountain, about six English miles off, whither they

Interior of Africa.

they were often in the habit of going. I have there spent one day with them, and declared that I meant to proceed for Tangier. They will accompany me to a certain distance, and give out at Mogadore that I am on my way to Tangier. As soon as I am left alone with my fellow-traveller, I mean to clothe myself in my Moorish garb, and to enter the great road which leads from Tafilet to Morocco. From thence I shall reach Deminit, a town situated at the foot of Mount Atlas, where I shall be safe from any searches which the governor of Mogadore might make, should he learn that I have not gone to Tangier. At Deminit, I shall join a caravan, which will pass there about that time, and with it I shall cross Mount Atlas, covered with snow, and next enter the burning plains of Tafilet. I shall remain at Tafilet with a German renegado. There are in that city a number of Germans. There are some Germans in Morocco, and to one of them I am indebted for some valuable information. I expect to find a German in Tombuctoo, and there I mean to remain six months, making it the centre of my observations on the interior of Africa. I shall pass for a physician: I have laid in a supply of medicines, of which I know the application. It is my wish to penetrate towards the south, and to be able to reach Wesemb, or the Cape. Should I find this too difficult, I mean to return to Europe to publish the journal of my travels; and shall again return to Africa, where I am destined to make some discoveries.'

X.

Description of a remontoire Escapement for Pendulum Clocks, invented by Mr. George Prior, *Jun*[*].

New escapement.

THE swing wheel, A, figs. 1 and 3, Plate III, has thirty teeth cut in its periphery, and is constantly urged forwards by the maintaining power, which, in the model represented in the engraving, is supplied by a small weight, X, figs. 2 and 3; CD are two spring detents, catching the teeth of the wheel

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[*] Soc. Arts, XXIX. anno. 1811. The society bestowed a premium of 20 guineas for this invention.

New escapement.

alternately; these are, at the proper intervals, unlocked by the parts marked 2 and 3, fig. 1, upon the pendulum rod H, intercepting small pins, *a b*, fig. 2, projecting from the detents, as it vibrates towards the one or the other; E is the renovating or remontoire spring, fixed to the same stud F as the detents. It is wound up by the highest tooth of the wheel, as seen in fig. 1, (its position when unwound being shown by the dotted line.) This being the case, suppose a tooth of the wheel is caught by the detent D, which prevents the wheel from moving any further, and keeps the renovating spring from escaping off the point of the tooth: in this position, the pendulum is quite detached from the wheel; now, if the pendulum be caused to vibrate towards G, the part of it marked 2, comes against the pin *b*, fig. 2, projecting from the renovating spring E, and pushes this spring from the point of the wheel's tooth; on vibrating a little farther it removes the detent D, which detained the wheel by the part 3 striking the pin (*a, fig.* 2) which projects from the detent; the maintaining power of the clock causes the wheel (thus unlocked) to advance, until detained by a tooth resting upon the end of the detent C, on the opposite side; by this means, the renovating spring will be clear of the tooth of the wheel as it returns with the pendulum, and gives it an impulse, by its pin *b*, pressing against the part 2 of the pendulum, until the spring comes to the position shown by the dotted line; in which position it is unwound, and rests against a pin fixed in the cross-bar of the plate; the pendulum continues vibrating towards I, nearly to the extent of its vibration, when the part 1 meets the pin in the detent C, and removes it from the wheel and unlocks it; the maintaining power now carries it forward, pushing the renovating spring E before it, until another tooth is caught by the detent D, which detains the wheel in the position first described, the renovating spring being wound up, ready to give another impulse to the pendulum.

N. B. The pin *b*, fig. 2, is not fixed to the renovating spring itself, but is part of a piece of brass, which is screwed fast to the renovating spring, and is made very slender near the screw which fastens it; this permits the end of the renovating spring to give way, if, by the weight being taken off the clock, or

New escapement.

any other accident, the escape-wheel should be moved backwards, so as to catch on the detents improperly.

The following observations are necessary to be attended to in this escapement.

1st. That the renovating and detent springs must spring from one centre, and as similarly as possible.

2d. That the force applied to the train must be so much more than what will wind up the renovating spring, as will overcome the influence of oil and friction on the pivots of the machine.

3d. That the renovating spring, when unwound, must rest against the point of the tooth of the wheel; which will be an advantage, as it thereby takes as much force off the tooth of the wheel resting against the detent spring, as is equal to the pressure of the renovating spring C, against the face of the tooth of the wheel.

4th. The detent springs must be made as slender and light as possible; though whatever force they take from the pendulum, by their elasticity in removing them, to unlock the wheel, so much force they return to the pendulum in following it, to where it removed them from; therefore action and re-action will be equal in contrary directions.

5th. That it is unnecessary for the pendulum to remove the detent or renovating springs, much farther than is necessary to free the teeth of the wheel, as it will always vibrate up to the same arc; in table clocks it ought to remove them further, so that it can go when not placed exactly level, or what is generally termed, out of the beat.

XI.

Description of a simple, cheap, and easy Method of preventing the Annoyance of steam from Boilers in Manufactories and other Places. *By* MR. GEORGE WEBSTER, *of Leeds*[*].

Easy means of conveying steam and vapor up a chimney.

THE introduction of steam into workshops and manufactories, is injurious to the articles, to the buildings, and to the workmen; and, when the matter evaporated from boilers is of an offensive nature, it must be still more desirable to dissipate, or carry it off, in the most efficacious and simple manner. Mr. Webster, after various trials, has accomplished this by an ascending trunk or pipe, which communicates with the chimney, and is explained in the following description, by reference to fig. 4. Plate III.

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[*] For which the Soc. of Arts gave their silver medal in 1811.

A A, the brick work surrounding the pan.

B, the steam chimney, made of wood, about two feet broad and six inches deep. A small opening at the back part of the pan admits the steam into this chimney; it may from thence be carried up to the top of the building, or turned into any smoke chimney near at hand.

In order to keep the water in the pan as hot as possible during the night, there are two dampers in the steam chimney at D, and if both these dampers are shut, and the whole top of the pan covered closely over at *c*, the boiling water, even when the fire is withdrawn, will keep hot for the workmen till the next morning.

C C, are loose boards, fitting close to each other, and covering completely the better half of the circle of the top of the pan; and upon this circumstance depends the whole secret of getting quit of the steam. If you remove these boards or partial coverings, the steam chimney loses all its use. The letter *b* shews the part of the top of the pan which should be left open to admit to the workmen a ready communication with the hot water; and through this open part a current of cold air is constantly seen to press and force the steam rapidly up the steam chimney.

It is proper to add, that there must always be an empty space of two or three inches between the surface of the hot water and the under part of the cover *cc*, so as to permit the steam to pass to the bottom of the steam chimney. To effect this purpose, and at the same time to allow the copper to be full of hot water, a rim or curb of wood F, about three inches thick, should be fixed on the top of the copper, and upon this the covering boards *cc* placed. This allows sufficient room for the steam to press forward to the steam chimney at all times.

The cover and wood steam chimney are removeable, and may serve for another copper, if both be not wanted at the same time.

METE-

XII.

METEOROLOGICAL JOURNAL.

1812.	Wind.	Barometer.			Thermometer.			Evap.	Rain	
		Max.	Min.	Med.	Max.	Min	Med.			
11th Mo.										
Nov. 25	S W	29·89	29·80	29·845	48	35	41·5			☽
26	E	30·07	29·77	29·920	47	43	45 0		0·26	
27	N	30·21	30·10	30·155	49	42	45·5			
28	N E	30·10	29 92	30 010	47	38	42 5			
29	S E	29·89	29·85	29·870	49	41	45·0			
30	S	29·95	29·88	29·915	50	47	48·5		0·15	
12th Mo.										
Dec. 1	S	29·96	29·72	29·840	52	44	48·0		5	
2	N W	30·08	29·96	30 020	49	42	45·5		—	
3	E	30·11	30·09	30·100	49	45	47·0		—	●
4	E	30 08	30 05	30·065	48	38	43·0		0·11	
5	E	30·22	30·08	30 185	44	33	38·5			
6	N E	30·51	30·29	30·400	42	26	34·0			
7	N E	30·51	30·41	30·460	35	23	29·0			
8	N E	30·41	29·94	30 175	34	18	26·0			
9	W	29·96	29·94	29·950	35	24	29·5			
10	N W	29 89	29·78	29·835	34	29	31·5		—	
11	E	30·00	29·97	29 955	36	27	31·5			☾
12	N E	29·97	29·79	29·880	32	24	28·0			
13	N E	29·79	29·71	29·750	34	24	29·0			
14	N E	29·71	29·66	29·685	35	28	31·5			
15	E	29·66	29·20	29·430	34	28	31·0			
16	E	29·20	28·98	29 090	34	28	31·0		—	
17	E	29 22	28·98	29·100	35	32	33·5		0.27	
18	E	29·51	29·22	29·365	38	33	35·5		0·18	○
19	E	29·57	29·47	29·520	38	35	36·5			
20	E	29·76	29·57	29·665	36	31	33·5			
21	N W	29·82	29·76	29·790	38	32	35·0		—	
22	Var.	30·02	29·82	29 920	42	33	37·5		—	
23	N	30·30	30·02	30·160	36	31	33·5		—	
24	N	30·46	30·30	30·380	35	32	33·5		3	
		30·51	28·98	29·882	52	18	36·68		0·95	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Eleventh Month, 28. The sky, about sunset, was overspread with *Cirrus* and *Cirrostratus* clouds, beautifully tinged with flame colour, red and violet. 30. a. m. The sky again much coloured.

Twelfth Month. 5. The weather, which has been hitherto mostly cloudy, with redness at sunrise and sunset, begins now to be more serene. 6. Hoar frost. 7. A little appearance of hail balls on the ground. 8, 9. Clear, hoar frost. 11. Snow this morning, and again after sunset. 13. An orange-coloured band on the horizon this evening; this phenomenon arises from reflection by the descending dew. 15. A gale from N. E. unaccompanied by snow, came in early this morning. 16. a. m. The wind has subsided to a breeze, and there now falls (at the temp. of 27·5°) snow, very regularly crystallized in stars. 17. a. m. It snowed more freely in the night, and there is now a cold thaw, with light misty showers. 18. A little sleet, followed by snow. Ice has been formed in the night, by virtue of the low temperature which the ground still possesses. A wet evening. 21. A little rain, a. m. 22. A dripping mist. 24. Cloudy; a little rain; some hail balls in the night.

RESULTS.

Prevailing winds easterly.

Barometer: greatest observed elevation, 30·51 in.; least 28·98 in.; Mean of the period 29·882 inches.

Thermometer: greatest elevation 52°; least 18°. Mean of the period, 36·68°.

Rain and snow 0·95 inches. The Evaporation during this period has not been ascertained.

PLAISTOW,
First Month, 7, 1813.

L. HOWARD.

XIII.

An Explanatory Statement of the Notions or Principles upon which the Systematic Arrangement is founded, which was adpoted as the Basis of an Essay on Chemical Nomenclature[*]*.*

By Professor J. Berzelius.

The affinity of chemistry explained.

WE may consider the affinity of chemistry as a tendency by which bodies are incessantly urged. By this affinity they are disposed to combine, in such proportions and in such numbers of each body, that they afterwards cease to manifest any farther affinity of combination, which property may from that period be considered as in a state of repose or inactivity. Such a combination or compound, which no longer shews any affinity towards the greater part of other bodies, may be itself called an *indifferent body*. If, for example sulphur, baritum, and oxigen, come into contact, they tend to combine in such a manner, as to produce sulphate of barytes; and in this compound the affinities of the ingredients appears to be in a state of repose; that is to say, they constitute an indifferent compound.

Absolute or complete union of principles,

The entire tendency or activity of the affinity is, therefore, exected to arrive, by an effect, which occupies a longer or shorter time, according to circumstances, at this state of repose or indifference. If the elementary bodies were collected at the same place, and all possessed an equally strong chemical affinity, an active chemical phenomenon would ensue, which would terminate in eternal repose. No force would tend to change this state of repose, and such different combinations as might be thus formed, being attracted by each other, by the means of gravitation and cohesion, would constitute a mass or aggregate of indifferent bodies.

does not appear in our observations.

But this is not the construction which takes place in nature, among the surrounding bodies of that small part of the universe which is submitted to our observation. A series of mutations take place in unorganized matters, by which organized nature is supported, and we have plausible reasons to conjecture, that a similar disposition prevails in the other parts of the immensity of the universe.

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[*] The Essay was published in the Journal de Physique, of Dr. Delametherie, Oct. 1811, or Tome lxxxiii. 253. It abounds with new and interesting observations; and it is with regret that I am prevented by its length from inserting it in our work.—The present memoir, which is of considerable extent, is taken from the Memoirs of the Academy of Stockholm for 1812.—W. N.

The circumstances which incessantly tend to destroy or to prevent the repose of combined elements, are light, caloric, and electricity, assisted by the circumstance that the chemical affinity of the different elementary bodies is not equally strong.

because prevented by caloric, light, and electricity.

Caloric, light, and electricity, have a mutual relation with each other; easy to be perceived, but very difficult to be comprehended. Very often the presence of one of these produces the other, without one being capable of determining whence it comes. When a large and powerful electrical pile is discharged by means of two points of platina, a sun is produced at the point of discharge; indeed upon a scale of infinite minuteness as to magnitude, but which, by the intensity of its light and heat, surpasses every other phenomenon of fire produced upon one globe; which fuses the metal, and loses nothing by the comparison, even when produced in the midst of a flame, supported by oxigen gas. The production of light and heat at the point of the electric discharge; that is to say, at the point where the two separated electricities it cease to manifest themselves as electricity, cannot be mistaken; and proves, that there is a relation between these substances, which we may, perhaps, hereafter be better able to comprehend than at present.

These have a mutual relation.

The union of electricities produces light and heat.

Caloric and the electricities exhibit, in our experiments, a kind of tendency to acquire an equilibrium; that is to say, to arrive at the same state of repose, which appears to be the ultimate end of the chemical affinity of ponderable matter. But this equilibrium of caloria and the electricities is incessantly broken by the rays of the sun, by which the surfaces of the planetary bodies is alternately enlightened at determinate intervals.

Tendencies to equilibrium of caloric and electricity,

There is therefore a process carried on in the sun, by which the repose of the united elements is incessantly intercepted or prevented, and which preserves them in a certain state of activity. It is impossible for us to determine the nature of this process; because the truth of ou rconjectures will never, in all probability, be proved in a satisfactory manner; but, notwithstanding the difficulty, it will always be a subject of interest

which is disturbed by the solar light.

to ascertain which of our conjectures may be the least improbable.

Common fire is produced; 1. By combination of bodies; 2. By union of electricities.

We know that the phenomenon of fire is produced on one globe on two principal or leading occasions. 1. When two bodies combine; for example, in oxidation, sulphuration, the combination of acids with bases, &c.; and 2. When the separated electricities mutually penetrate each other, and cease to appear as electricities.

Friction and concession.

(There are, nevertheless, two other manners by which fire may be produced; namely, friction and compression. As to friction, there is reason to believe that it will be found to class itself along with the electric discharge; and compression, on the other hand, does nothing more than drive the caloric out of a body which it contains already produced. But in the present part of our discussion, we attend only to the cases in which caloric appears to be produced, that is to say, in which we cannot conceive whence it comes).

The sun is in a state of combustion,

It is incompatible with every scientific notion we possess, that the phenomenon of an interior fire should be produced in the sun, by a chemical combination, or by a condensation of ponderable substances. Such an opinion has been rejected by our ancestors, though their notions of combustion were less precise than ours; and it appears to be contradicted by the circumstance, that the magnitude of the mass of the sun remains constantly without alteration, at least, as far as our observations can determine. It remains therefore as the least improbable of our conjecture, that a process is affected in the sun, analogous to that which obtains between the points by which an electric pile is discharged; and we must imagine that this process, when once commenced, must, from the nature of the actual arrangement of things, continue for ever; and that, consequently, the activity of created matters is maintained, as it were, by a gyration in a circle, or by always returning again to their first situation or state, as in astronomy we know to be the case with their motions in space. It is beyond the limits of human reason to determine how these processes at first began, and it would no doubt be unworthy of an enlightened and discerning mind to presume seriously to form any conjecture upon the subject.

and may therefore be supported in a state of continued electric discharge.

The electrici-

Our experiments with the electric pile, have proved how much the Electricities are concerned in the operations of chemical affinity; and that sometimes they are suppressed, and at other times made to act in an opposite sense. It was even observed, before the discovery of the Electric pile, that the Equilibrium of Electricity is sometimes disturbed by chemical operations, and the knowledge acquired from the labours of the last ten or twelve years, has shown us, that there is not a single action of affinity, in which the electricities do not co-operate.

ties are concerned in the operations of affinity,

We do not know how this co-operation is made, and, for the moment, we must be satisfied with conjectures upon it. What we with certainty know is, that two bodies which have affinity for each other, and which have been brought into mutual contact, are found upon separation to be in opposite states of electricity. That which has the greatest affinity for oxigen usually becomes positively electrified, and the other negatively. Bodies which have little affinity between them, or, which have nearly an equal affinity for oxigen, do not sensibly derange the electric equilibrium by their mutual contact. This is not only the case with combustible bodies, but it also takes place with the oxides; as for example, the oxalic acid, dry and deprived of its water of crystallization, brought into contact with quick lime, becomes, according to the experiments of Davy, negatively electric, while the lime becomes positive. And since the electric state of these bodies is more marked, the higher the temperature, that is to say, as the chemical affinity becomes more active; and lastly, as at the moment of their union there is a production of heat, which may vary from a very slight elevation of temperature to that of the most intense fire, we think we may conclude, that at the moment of the chemical combination, there is a discharge of the opposite electric state of the bodies, which here, as in the pile, produces the phenomenon of fire, at the instant when the electricities disappear.

in a manner not known;

but bodies having affinity, shew the electric states on separation,

that bodies becoming positive which has the strongest affinity for oxigen.

Elevated temperature increases the affinities and the electricity, and at the instant of union, heat ensues. probably from an electric discharge.

A derangement of the equilibrium of electricity appears therefore to precede, and as it were predispose, the action of the chemical affinity; though this phenomenon from physical reasons cannot be always discovered by our instruments; as may happen, for instance, when one of the bodies is in the liquid state. Davy found, in conformity with this, that sulphur, heated upon

A change in the electric equilibrium precedes the action of ch. affinity.

upon copper, gave signs of very strong electricity, constantly increasing with the temperature till the sulphur melted, at which instant the signs disappeared,

After ponderable bodies have combined, they can be separated only by the electricities acting peculiarly on each body.

After the union of ponderable bodies, in which the electricity is seen to fly off in the form of light and heat, the ponderable bodies are reduced to a state of chemical repose. The elements of the combination can no more be separated, nor be restored, to their original form and characters, without the influence of a mass of electricity, in a state of charge or of separation, as in the operation of the pile. But in this case the electricities, tending to regain their equilibrium, decompose the combination, by operating each upon its relative constituent part to which it restores its original form and characters.

(To be Continued.)

XIV.

Facts and Remarks, upon the Interruption which the situation of the maintaining weight produces in the rate of a clock, when near the pendulum. By H. K.

To Mr. Nicholson.

SIR,

Rate of clocks affected by the position of the weight,

IN your Journal for October last, I observed a paper by Mr. Thomas Reid, on the effect produced on the going of clocks, by the attraction between the weights and the pendulum.

was observed and remedied by professor Hornsby.

The effect alluded to, viz. that of the arc of vibration becoming less when the weight is near the ball of the pendulum was remarked some years since, by the late Dr. Hornsby. This gentleman having done me the honour to accompany me in a visit to the observatory at Oxford, pointed out an astronomical clock there, the weight of which he had contrived to pass behind the clock case. He informed me, that he had remarked an irregularity in the going of the clock, when the weight approached the ball of the pendulum, and attributed it to the increased resistance of the air, from its free motion being impeded by the weight of the clock.

Reason why this effect is not to be ascribed to attraction.

Indeed, it does not seem that attraction could produce the effect alluded to; for, though the ball of the pendulum might be retarded in its ascent, its motion would be proportionably accelerated on its return.

I have been induced to trouble you with this, merely from respect to Dr. Hornsby's memory, and not with the slightest intention of depreciating the talents of Mr. Reid.

I am Sir,

Your obedient humble Servant,

H. K.

Ipswich, Dec. 6, 1812.

REMARK.

Additional obs. on Mr. Reid's communication.

From the nature and tenor of Mr. Reid's communication I concluded, that his single weight descended either in front or behind the ball, and not on one side of it; and in this arrangement its attraction would add to that of gravity, whether perceptibly or not. I likewise requested his brother, who brought the paper, to suggest that it might be desirable to make trial of a temporary piece or mass, to be put on or taken off at pleasure, in the place where the weight had been inferred to produce the greatest acceleration; and to keep the weight out of the limit of disturbance; this would remove all suspicion of irregularity in the train: And I would, from the ingenious observations of my Correspondent, suggest farther, that the temporary piece should be a thin shell of brass, with a solid core of lead; which, when taken out, would greatly diminish the attraction, but not the impediment from increased resistance of the surrounding air.

SCIENTIFIC

SCIENTIFIC NEWS.

Mountains of Lapland, &c.

Valenberg's Journey for examining the mountains of Lapland.

AN *Account of a Journey*, undertaken in 1807, by M. Valenberg, has been published lately, at Stockholm, under the auspices of the Academy of Sciences of Sweden, *for the purpose of determining the height of the mountains of Lapland, and observing their temperature.* The mountains visited by M. Valenberg, make part of the great chain which runs through Sweden and Norway, and stretches in some of its branches even to Finland and Russia. They are situated between 67 and 68 degrees north latitude, and belong to the polar regions. On several points their bases are washed by the sea, and from their summits the immense plain of the Northern Ocean is discoverable. These mountains had been only hitherto viewed in all their majestic grandeur by the Lapland nomade, following his flocks of deer and his game. A few travellers had contemplated them at a distance; and M. de Bruck, a learned German, during his travels in Norway, approached within a short space of them; but no person had ever yet penetrated into this asylum of nature, and attempted to struggle with the difficulties of ascending these summits, eternally covered with snow and ice.

The undertaking was difficult in many respects. The ascents were mostly excessively steep, and in climbing them the traveller was by turns suspended over deep fissures, lakes, torrents, bottomless marshes, and gulfs. He had no intelligent guide, there was no habitation on his route, and no assistance to be expected. He frequently was obliged to make circuits of many leagues to reach a summit; and he crossed not only snow and ice full of crevices, but also marshes, where he ran a continual risk of being buried in the mud and stagnant water. He passed the nights on naked rocks, without a tent or the smallest shelter; and he was frequently reduced to quench his devouring thirst by swallowing snow, which occasioned him inflammations and painful suppurations in the mouth.

M. de Valenberg's measurements give the Lapland mountains an elevation of from 5 to 6,000 feet above the level of the sea. Although this elevation is less than that of the mountains of Switzerland and the Pyrenees, all the phenomena of the Alpine regions, and particularly glaciers, are observable. At such a proximity to the polar circle, the region of eternal snow commences at nearly 4,000 feet above the ocean, while in the Alps it begins at from 7 to 8,000, and in the Pyrenees at 8,000 feet.

On the 14th July, M. de Valenberg ascended the most considerable glacier, called *Sulitelma*, a Lapland word, which signifies Solemn Mountain, because formerly the Laplanders adored on one of its summits their principal idol. This mountain, which is the *Mount Blanc* of the north, is composed of a succession of summits, of which the base has an extent of several leagues. Its greatest elevation is 5,700 feet above the sea. To reach this elevation, our traveller was obliged to make his way over enormous crevices, where recently before some hunters had been engulphed with their deer and their dogs. Seas of ice have descended into the vallies 700 feet below the line of snow. There is a border of earth surrounds the ice, consisting of slime and stones. The ice of Sulitelma is very clear, and almost transparent; it is as hard as stone, but not so heavy as the ice of the sea. The traveller gives several details respecting its internal composition, the figures by which it is characterized, and the crevices formed on it. The snow is sometimes 100 feet in depth, and so hard that the footsteps leave no mark on it. That which is detached from the summits, or crevices, roll to immense distances. Fortunately, these avalanches in their descent act only on inanimate nature: whatever direction they take they seldom encounter living beings, or the abodes of men. All is desert in these regions for vast extents, where industry has gained no conquest over the solitary domain of the primitive creation.

The traveller terminates his account by general considerations on the temperature, and by tables of meteorological observations. He determines with precision the different regions of the mountains, and characterizes them by the productions which he found there. In proportion as the line of snow is approached, the productive force of nature diminishes, and men, brute animals, and plants, yield to the rigour of the cold. At 2,600 feet below the line, the pines disappear, as well as the cattle and habitations. At 2,000 feet the only tree is the birch; and its degraded form and indigent verdure attest the inclemency of the climate; at the same time the greatest number of wild animals disappear, and the lakes contain no fish. At 800 feet below the same line of snow, the Laplander's progress is stopped for want of moss for his rein-deer. Above the line every thing presents the picture of agony and death. The most robust lichens are only to be found at 1,000 and 2,000 feet, in the crevices of perpendicular rocks; and the bird named *emberiza nivalis*, or snow-bird, is the only living creature to be seen. The heat does not rise to one degree of Réaumur, in the region, which is 5,000 feet above the sea.

Mr. Fiddler, a captain in the Hudson's-bay service, has communicated to Mr. Arrowsmith, the draught of the district of country which lies between the rocky mountains and the great ocean, and between the latitude 52 and 46. It contains all the head waters of the Columbian River; of a lake, called, by Mr. Fiddler, Lean's Lake; a river running into it, called Arrowsmith's River; and a river of magnitude, called Wedderburn's River. The whole tract is inhabited by tribes of flat-head Indians, otherwise called Têtes de Boules, and one large extent is filled with wild horses. Mr. Arrowsmith purposes to introduce these discoveries into his General Map of North-American Discoveries.

Mr. Arrowsmith has completed a new Map of Germany, in six sheets of double elephant, being the largest map of that empire ever drawn and published in England. Like all the maps of this eminent geographer, this new one is derived either from original or unquestionable and superior sources.

The same geographer has for some years been engaged on a Map of England and Wales, in 18 sheets, which, when put together, will be 10 feet by 12. Of this extraordinary map it deserves to be noticed, that it will contain at least 1,000,000 names, which is the more remarkable because the places enumerated in the Population Return are only 15,741; and Capper's Topographical Dictionary does not contain above 20,000 places for the three kingdoms, although double the number contained in Luckombe's Gazetteer.

It is with regret that I find myself under the necessity of taking notice of some passages in the preface to Dr. Thompson's Annals of Philosophy, in which he animadverts upon the English Philosophical Journals.

1. Of my Journal he says, "that for several years it was "excellent," and adds, "that for some years past, *if report "says true*, it has not been the property of the original editor, "but of a bookseller; and, in reality, edited, not by Mr. "Nicholson, but by some unknown person employed by the "bookseller.

2. Of the *Philosophical Magazine*, which he calls a rival publication, he says "it is edited by Mr. Tullock, a printer "from Glasgow, and publisher of the evening newspaper, "called the Star," and that "it, perhaps, never contained so "much original matter as my Journal.

3. Of the *Repertory of Arts*, he says it consists chiefly of specifications of patent inventions, with a few additional papers copied from the Transactions, or other Journals; but he overlooks the remarks and discussions from the inventors and others, which are inserted in that work.

4. And of the *Retrospect* of Discoveries; or Abridgment of periodical and other Publications, he says, that it is, as the title implies, *merely* an abridgment of the other three Journals, of the British Transactions, and of *one or two* French periodical works. But, in so doing, he denies the existence of those numerous, clear, and able criticisms upon the subjects so abridged, which constitute part of the plan of the Retrospect, and are every where to be met with.

5. His deduction then follows: "Such," says he, "being "the state of the English Philosophical Journals, our readers "will not be surprized that we (Dr. T.) venture to offer our "claims to the attention of the public."

Whether it became Doctor Thompson to have assumed the office of Censor, with regard to the productions which he appears to consider as rivals; or whether it would have been more decorous for him, as a man offering himself in the venerable presence of the public, to have felt the consciousness of human infirmity, and expecting to have his own faults viewed with candour, to have avoided the volunteer task of exposing those of others. Into these points I do not enquire; and, if these had been the only objects of question, I should have been silent.

But I can enquire and can decide, that it did not become Dr. Thompson to endeavour to depress his rivals by stating or giving currency to untruths. This is a point of moral character which I will treat in no other way than by shewing he has done so. My statements are numbered correspondently with the paragraphs at the beginning of this Notice.

1. The Philosophical Journal was published for the first year, 1787, for the joint account of myself and Messrs. Robinsons. In 1788 the entire Copy-right became mine and has continued so without interruption ever since. No bookseller ever had power to employ, or did employ any person in editing or interfering with the copy of the Journal That copy has been provided by myself and Correspondents; and I have always had one assistant, fully acquainted with the Sciences, and Languages, of my own knowledge and appointment, and not employed by a Bookseller. My name as Author and Proprietor has every month been before the Public: was it not the duty of a good man, instead of sheltering himself under the words, "if Report says true," to have enquired whether the Report was or was not true, before he ventured to join in propagating it?

2. I am informed that Mr. Tilloch (not Tulloch) was not a *printer* at Glasgow, and is *proprietor* (not publisher) of the Star. These are unimportant sounds in themselves; but they shew the disposition of Thompson to lower his supposed opponents, and his want of accuracy and correctness. All the world knows how many eminent men have been printers, and how little in a nation like ours, the science and acquisitions of men, depend upon their pursuits in business.

3. His prejudiced notice of the Repertory speaks for itself:

4. And so does his positive assertion that the Repertory is *merely* an abridgment. He could not but have seen, though he has thought fit to deny, the excellent original discussions it contains.

5. His summary deduction points out the spirit and motives of his statements; namely, to shew that the English Journals are bad, and, by inference, that his own will be much superior.

My principal object has been to expose the Doctor's conduct with regard to myself. The world must determine for him, whether that conduct can promote any interest for which a well-disposed mind ought to be solicitous.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1813.

ARTICLE I.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor J. Berzelius.

(*Continued from p.* 146.)

IN the explanation of these phenomena, there is a circumstance which confounds us more than all the others, viz. that there is only one body, that is to say oxigen, which possesses absolute and invariable electro-chemical characters. All the others, while they manifest a fixed and determinate relation with regard to oxigen, vary with regard to each other. Sulphur, for example, is positive with regard to oxigen, but is negative with regard to the metals; arsenic is positive with regard to oxigen and to sulphur, but it is negative with regard to the other metals; silver is positive with regard to oxigen, sulphur, and arsenic, but is negative with regard to most of the metals.

Oxigen alone possesses absolute and invariable electro-chemical characters.

[The author here subjoins the following annotation, which, on account of its extent, I have printed in the body of the page.—W. N.]

Though it may, perhaps, be too early for us to adopt any notions respecting this difficult subject, I shall here offer a con-

Theory of electro-chemistry.

jecture upon the manner according to which the whole of the effects may take place, without contradicting any of the results we possess concerning electricity.

Atoms possess electrical polarity; on the intensity of which their affinities depend.

Admitting, that bodies consist of particles or atoms placed near each other, in such manner as may appear from their property of combining in proportions of their multiples, we may consider these atoms as possessing an electrical polarity upon the intensity of which the force of their affinity depends. In this case the chemical affinity becomes identified with electricity, or rather the electric polarity. In order to explain the different electro-chemical characters, we must add to the general polarity a kind of specific unipolarity, by means of which one of the poles contains more of the + E, or of the — E, than the opposite electricity in the other pole is capable of saturating. A body of which the positive pole predominates, that is, which contains an excess of positive electricity, constitutes an electro-positive body, and vice versâ. Many bodies require an elevation of temperature to enable them to act upon each other. It appears, therefore, that heat possesses the property of augmenting the polarity of these bodies; and that the difference in activity of the affinity at different temperatures, appears to depend on the same cause, in like manner as the force with which a combination preserves its existence, appears to depend on the intensity of the electric polarity when this is at its maximum, or rather the intensity of that polarity at the moment the combination is made. This circumstance explains why the phosphoric acid is decomposable by charcoal at an elevated temperature, although phosphorus decomposes the air of the atmosphere at a temperature at which charcoal has no influence upon that fluid.

One pole may be stronger than the other.

The body is called electro-positive, or electro-negative, from its predominating pole.

Heat augments these electricities.

In the theory of atoms, there is some difficulty in conceiving the difference between the juxta-portion of homogeneous particles, separable by mechanical means, and that of the heterogeneous particles, which produce a new particle, very seldom decomposable by means purely mechanical. The hypothesis of polarized atoms assists us upon this occasion. The cohesion of homogeneous particles may be compared to the juxta-position which we observe in the electrophore between the opposite electricities of the metallic plate and the resinous surface Contact keeps them in a state of charge or neutralization; which,

The particles of homogeneous masses appear to unite like the plates of the electrophore.

which, in fact, is simply juxta-position, and is destroyed when the surfaces are separated, and each appears again in possession of its original electric state. When heterogeneous atoms combine (whether the combination do consist simply in juxta-position, or, which is more difficult to comprehend, in a partial or total penetration) they appear to adjust or dispose themselves so as to touch with the opposite poles; of which the electricities produce a discharge which causes the phenomenon of elevation of temperature, almost constantly apparent at the time of any chemical combination, and the particles remain combined until their discharged poles are, by some means or other, restored to their former electric state.

Heterogeneous particles seem to unite by a discharge of electricity with production of heat.

As we know, from fact and experience, that bodies of the same electro-chemical class (that is to say, bodies in which we conceive that the same pole predominates) can combine, it appears, that the force of affinity depends rather on the intensity of the general polarity, than of the specific unipolarity; and from this reason it may be, that sulphur has more affinity with oxigen, than gold or platina has, although sulphur has the same unipolarity as oxigen, and those metals have an opposite unipolarity to that of oxigen.

The affinity depends more upon the intensity of the general polarity, than upon the excess in one pole beyond the other.

It is clear, that when two bodies, in which the same pole predominates, combine together, the new particle must possess their unipolar force concentrated in one of its poles, and must, consequently, have electro-chemical properties more intense; and this is a good reason why sulphur and oxigen produce the strongest acid. On the contrary, when particles possessing an opposite polarity unite, the polarity of one of the particles most frequently predominates; for example, in potash, and in most of the metallic oxides, the predominating pole of the metal also predominates in the compound. In some instances, the product is a neutral compound, in which neither of the poles predominate, such as the superoxides: in other instances, the pole of the metal predominates in one degree of oxidation, and that of the oxigen in another.

When bodies, having the same predominating pole, combine, the electro chemical property of the compound will be more intense; and vice versâ.

The combination of polarized atoms requires a motion to turn the opposite poles to each other; and to this circumstance is owing the facility with which combination takes place when one of the two bodies is in the liquid state, or where both are in that state; and the extreme difficulty, or nearly impossibility,

Polarized atoms must turn to each other in order to combine. This will be easily done if one or both be fluid. Solids can scarcely combine. The atoms of gases have less electric action because too remote.

of effecting an union between bodies, both of which are solid. And again, since each polarized particle must have an electric atmosphere, and as this atmosphere is the predisposing cause of combination, as we have seen, it follows, that the particles cannot act but at certain distances, proportioned to the intensity of their polarity; and hence it is that bodies, which have affinity for each other, always combine nearly on the instant when mixed in the liquid state, but less easily in the gaseous state, and the union ceases to be possible under a certain degree of dilatation of the gasses, as we know by the experiments of Grothuss, that a mixture of oxigen and hidrogen in due proportions, when rarefied to a certain degree, cannot be set on fire at any temperature whatever.

The pile restores the atoms to their former state.

The chemical action effected by the discharge of the pile, consists in the particles in a combination being re-polarized. In a combination of particles having the same unipolarity, the pile merely restores, by the decomposition, the general polarity, because their specific unipolarity was not changed by their union; but in combinations of opposite unipolarity, it likewise restores the specific unipolarity of the elements. May we conclude, that, in the first case, the general re-polarization takes place in the same manner as the loadstone gives magnetism to a small particle of steel, and that in the second, the pile contributes, by its own specific energies, to restore the predominating poles.*

[Here the annotation concludes.—W. N.]

Classification of bodies as to their disposition to be collected round the poles of the pile. *Electro-*

In my essay upon chemical nomenclature, I have divided bodies into electro-positive and electro-negative, the first of these denominations being appropriated to bodies which, by the action of the pile, are collected round the positive pole, and vice versâ, I have noticed the probability, that these names had been employed in the opposite sense; and my subsequent reflections upon this object, obliges me, at present, to change these denominations for each other. I shall, therefore, hereafter call those bodies *electro-positive* which are collected round the negative pole, and those *electro-negative* which are collected round the positive pole. With regard to the electro-chemical relations of bodies mutually, I shall divide them into five different classes.

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* In this beautiful generalization of facts, which promises to become more conclusive the more it shall be studied, this last paragraph seems rather obscure. The poles of the voltaic pile appear to present, to the principles of a compound, points of attraction more powerful than that which maintained the combination; and they transmit the electric energy from particle to particle, so as to complete the total decomposition. But we do not yet appear to possess analogies to carry us much farther.—N.

positive attach to the negative, and *electro-negative* to the positive pole.

1. *Absolutely electro-negative*; oxigen alone.

1. Absolutely electro-negative.

2. *Electro-negative in general*; all combustible bodies which produce acids with oxigen, are constantly collected at the positive pole of the pile. To this class the metalloids belong, and among the metals arsenic, molybdena, and wolfram.

2. Electro-negative.

3. *Bodies of a variable electro-chemical nature.* This class includes (*a*) such bodies as, when combined with oxigen, are electro-positive with regard to the preceding class, but electro-negative with regard to the bodies which constitute the last of the subsequent classes; (*b*) such bodies as, in one degree of oxidation, constitute a saline base, and in another degree an acid. Tellurium is an example of the first, and antimony of the latter.

3. Variable.

4. *Indifferent.* Oxided bodies, which possess no decided character, being neither acid nor saline bases. Such are the oxides of tantalium and of silicium. This class likewise includes the combinations of acids with saline bases, that is to say, the salts.

4. Indifferent

5. *Electro-positive.* Combustible bodies and their oxides, which, during the action of the pile, are never collected round the positive pole, and of which a great part, when combined with oxigen in excess, instead of forming acids, produce superoxides. Such are potassium, baritium, lead, silver, &c.

5. Electro-positive.

It is proved by experiment, that the more opposite the electro-chemical nature of two bodies is, the stronger in general is their mutual affinity. A combustible body consequently tends with greater force, to combine with oxigen, than with any other combustible body with which it may have affinity. Hence we may conclude, that, if it were possible to obtain pure oxigen in the solid form, and if, in that state, it were put into contact with a combustible body, it would become much more strongly electric than, for instance, sulphur with copper, and would, in fact, produce, in combining with the combustible

Mutual affinity being stronger, the more opposite the electro-chemical nature of bodies Combustibles will unite strongly with oxigen,— and produce elevation of temperature

by a process resembling the voltaic discharge:

body, an elevation of temperature much higher than could be produced by the combination of any other body with the same combustible. These reflections appear to indicate that, in the phenomenon of combustion, as in general in every chemical combination, the phenomenon of fire is produced by a cause analogous to that which is manifested on the occasion of the discharge of the electric pile; that is to say, by a discharge between the opposite electricities of the oxigen and of the combustible body, which is made at the moment of combination.

and this will depend more on the strength of affinity, than on change of density, &c.

The same considerations also explain why the phenomenon of fire is more intense accordingly as the affinity of the bodies which combine is more powerful (varying from the slightest elevation of temperature to the most intense fire) without any remarkable relation between the expansion or condensation the bodies may have undergone from their union.

Hence the effects of sulphuration are similar to those of combustion:

This electro-chemical view explains what was so difficult to be comprehended in the time of our predecessors, namely, how sulphuration could produce a phenomenon of fire exactly similar to that produced by combustion; and it classes together all the disengagements of caloric or fire, occasioned by chemical combinations. As it explains, in a consistent manner, that which the old theory could not account for, it appears to deserve our confidence, or at least our attention. I shall explain my notions by an example.

And charcoal between the poles of the pile in hidrogen or azote, is strongly ignited as in oxigen; but there is no combustion.

If any very powerful electric pile be discharged by pieces of charcoal in hidrogen or azote gas, we see the charcoal become ignited, and produce the same phenomenon, as if it were actually burning. A spectator, who, on this occasion, had no knowledge of the influence of the pile, would say that the charcoal was burning. But, nevertheless, there is, in this case, neither oxidation nor chemical combination of any ponderable matter with the charcoal, and, notwithstanding this, the phenomenon of fire is the same as if it had been produced by combustion. Now, it appears to be a well-founded conclusion, that the same effects are produced by the same causes; that is to say, that the fire in each of these cases is produced by an electric discharge.

The oxigen is not *condensed* by burning charcoal; and,

Charcoal does not condense oxigen by burning, but, on the contrary, is dissolved in the gas of which the volume undergoes no change. We cannot, therefore, assert, that the caloric

of

consequently the heat is not caused by condensation,

nor by change of capacity.

of combustion of the charcoal is the effect of a condensation, namely, that the oxigen gas has parted with the caloric which was employed in maintaining its gaseous form ; and it is clear, that the fire owes its origin to some other circumstance. Those who may not be disposed to approve the electro-chemical explanation, may, observe, that the fire in this combustion is produced by the difference between the specific heats of oxigen gas, and carbonic acid gas. But, although it cannot be denied, that such a cause (or incident) may contribute to (or accompany) the production of heat, it can be easily shewn, that it is not the principal or general cause ; because the nitric acid in which the oxigen still preserves its property of producing fire with a number of combustible bodies, possesses as little specific heat as the carbonic and the sulphuric acids. In like manner, the difference of specific heat between the metallic sulphurets and that of a metallic body, is too inconsiderable to afford a plausible reason for the fire produced by sulphuration.

Electro-chemical cause of heat, in simple elective affinity, elucidated.

When a combination already formed, as, for instance, between A and B, is decomposed by the more powerful affinity of a third body C, so that this last separates A from the combination AB, and forms CB—such a decomposition is usually accompanied with an elevation of temperature, or even with fire ; and this elevation is greater the more considerable the difference may be between the affinities of A and of C to B. We may form a notion, that this effect is owing to a more perfect neutralization of the electro-chemical properties of the constituent parts in the new, than in the old combination. If, on this occasion, B were oxigen, and A and C two combustible bodies, the electro-chemical nature of B must be admitted as more perfectly neutralized by C than by A ; and at the instant when A is reduced to its original combustible state, it receives from C, which loses its like state, a quantity of positive electricity, equal to what it had lost when it entered into combination with B.

The properties of compounds are remarkably affected by the electric nature of their components, e. g. sulphur

When bodies combine with others, in some instances more positive, and in others more negative, than themselves, are found after these two circumstances in very different states ; as sulphur, for instance, is in a quite different state in the sulphuric acid, from that which it possesses in the sulphuret of lead. From the former it can be disengaged by a number of electropositive bodies, and oxigen, which are both neg. form sulph. acid and the sulphur may be disengaged by many el. positive bodies :—but in sulphuret of lead no el. pos. body can take the lead; but an e. neg. body, viz. oxigen is required.

Cause why sulphur and phos. are rendered more combustible by union with alk. The latter is el. pos. and renders the sulph. less neg. and therefore more disposed to oxigen.

An opposite fact.

bodies, but from the last it cannot be disengaged by the affinity of any electropositive body to the lead; but for that purpose the affinity of another body, more electronegative than itself, namely oxigen will be required. Sulphur has, therefore, occasion for opposite electricities, in order to effect its separation from these two different combinations. It is well deserving of attention, that when such an electronegative combustible is combined with an electropositive oxide, the combustibility of the former (or its electropositive relation as to oxigen) is considerably increased; probably because its electronegative dispositions have been destroyed by the positive electricity of the oxide. We observe this in the great oxidability of sulphur and of phosphorus, combined with the alkalis, or alkaline earths. In a combination of two combustible bodies of opposite electrochemical natures, this augmentation of combustibility does not take place, and the combination of the two is less combustible than that one of the constituents, which was the most so, because one of them has lost exactly as much of its electropositive characters, as the other (the electronegative) has lost of its characters; and as in this case the effect must result from the sum of the affinities, it follows that the affinity of the most considerable is diminished in proportion as the quantity of the other is greater, and its affinity for oxigen less. It is from this explanation that we may conceive a phenomenon of which I shall give an account; namely, that the oxide of tin mixed with the oxide of gold, becomes reduced to the metallic state without the addition of a more combustible body; simply by the action of heat, by forming a metallic alloy of gold and tin, which is not decomposable by fire, even when fused with saltpetre.

Heat discomposes bodies; whence probably caloric may become electricity and restore the orig. properties of the particles.

Heat often produces, without the co-operation of other circumstances, a decomposition of combinations; and as in the electrochemical theory we form the conclusion, that no body can be restored to its original properties without the influence of the same electricity which it parted with when it entered into combination, we must likewise imagine as a consistent consequence of this fact, that in the same manner as the separate electricities, by their combination disappear and produce fire, so caloric in its turn, when accumulated and tending to regain its equilibrium, is capable, in certain circumstances, of disappearing as caloric, and re-appearing in the electric state restored to the separate elements.

If the chemical affinity be nothing more than the result of the polarity of the particles, it will follow decidedly, that it cannot be affinity which is the first mover, and causes the electric phenomena; but that on the contrary, the play of chemical affinities in the pile must be a consequence of these last: And this opinion is accordingly confirmed by experiment.*

The phenomena of the pile are not caused by affinity?

[Annotation. See remark at the note on page 154.]

* I was long of opinion that the oxidation of the zinc in the electric pile, was the cause of the change of electricity, and I endeavoured to prove that this hypothesis was sufficient to explain the phenomena of the pile (See my Theory of the Electric Pile in the "Neues Allgemeines Journal der Chemie," by Gehlen, in the year 1807.) But the experiments of Davy and Pfaff, having rendered my opinions less probable, I endeavoured to convince myself of the truth, by an experiment which I think decisive. I took 12 tubes of glass, half an inch in diameter and three inches in height, and closed at one end, I half filled them with a strong solution of the submuriate of lime (such as is obtained by the residue after the preparation of caustic ammonia) and above this fluid I poured diluted nitric acid, with the precaution not to mix the liquids. I ranged these tubes in succession, and then took copper wires, round one of the extremities of each of which I had melted zinc, in order to attach a knob of that metal to that end. I immersed the zinc-coated ends of each into one of the tubes to the bottom of the submuriat, and then bended the upper ends of the respective wires, so as to immerse them into the middle of the acid of each nearest tube. This arrangement, consequently, formed a pile in the order following: copper, zinc, submuriate of lime, nitric acid; copper, zinc, &c. It is evident that the chemical affinity which produces oxidation at the common temperature, was here at the surface of that part of the copper, which was in contact with the nitric acid; and that if this oxidation had been the primary cause of the electricity of the pile, the *pole of copper* in this construction *ought to have possessed the same electricity* (namely the positive) *as the zinc pole in the common pile.* Before the poles, or extremes, of this small pile were connected, the copper continued to be constantly dissolved in the

Experiment to shew that the ordinary chemical actions of bodies do not produce that phenomena.

By a row of tubes a pile was made in which the order was copper, zinc, submuriate of lime, nitric acid; copper, &c. While the extreme poles were unconnected, the nitric acid dissolved the copper, and the submuriate had no effect on the zinc. If oxidation were the cause of positive el. the copper would here have been pos.

acid,

but when the extreme poles were joined, *the action in the tubes was instantly reversed.* The zinc was then oxided, the copper ceased to be dissolved and the pole on the zinc side was positive as usual,

the elect. was not therefore governed by the action of the ordinary simple affinity.

Changes in the liquids.

acid, which it turned blue, and the surface of the zinc remained metallic and without any perceptible change. And lastly, I combined the poles by means of silver wires, passed into a tube filled with a solution of muriate of soda. But I was greatly surprised to find the effect *directly contrary* to what the theory which considers oxidation as the cause of the electricity of the pile had led me to expect. The solution of the copper instantly ceased and the zinc became covered with a mass of white oxide, vegetating on all sides in the form of wool. The pole of the copper produced hydrogen gas as usual, and the zinc pole caused an abundant precipitate of muriate of silver. The electric state, therefore, produced in this case an affinity, which, at the ordinary temperature of the atmosphere is inactive, and caused another very active affinity to cease, which was already in operation; and this could be effected by no other cause than that of the electricity produced by contact, which occasions the electric charge of the pile, and disposes the affinities which shall be put into activity. This little electric pile, was very powerful, and disengaged so large a quantity of gas, as would not have been exceeded by 100 pair of plates. But what could be the cause of this ?—I exchanged the submuriate for neutral muriate; it then produced a very moderate effect, corresponding with the number of pairs; and lastly, I substituted neutral muriate of zinc instead of the muriate of lime, and then the effect was scarcely perceptible, though it continued sufficient to prevent the oxidation of the copper in the nitric acid, and to shew that the conductor of the zinc pole continued always to be oxided. It appears, therefore, that the activity of the pile depends on the liquid substance, which during the process must change place; and that the most advantageous construction of a pile is copper, zinc, alkaline substance, acid, copper, zinc, &c. The pile will continue active until the order becomes inverted, that is to say, copper, zinc, acid, alkali, &c. This experiment also proves how necessary it is in every theory of the pile to attend to the chemical effect which must take place in the liquid.

[Here the Annotation ends.]

The electricity excited in the pile operates by affinity to great distances.

The connection between chemical affinity and the electric state, is also such, that upon every occasion wherein the effect of the electricities, excited by contact, cannot take place, the affinity acts only at a distance, infinitely small and impossible to

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be determined; but on the contrary, whenever that effect can manifest itself, it acts to very considerable distances, as for instance, in the precipitation of metals upon each other; and it is very probable, that actions at a distance produced by the electric powers do take place in the bowels of the earth, and contribute not only to the great revolutions of which we find the astonishing vestiges, but likewise to the tranquil formation and decomposition of minerals.

From the electrochemical view of nature we also derive a correction in our notions concerning the principles of acidity. The celebrated Lavoisier having found that sulphur, phosphorus, charcoal, arsenic, &c. produce acids when combined with oxigen, considered oxigen as the acidifying body. But notwithstanding this conclusion is supported by the circumstance that oxigen is the most electronegative of bodies, and the acids are also electronegative bodies, bodies were afterwards discovered, possessing the principal characters of acids without containing oxigen; and after the discovery that the saline bases are also oxided bodies as well as the acids, it would be equally incorrect to attribute the acid characters to oxigen, or some of its combinations, as to suppose it to be the principle of basidity, or to call it the alkaligenous principle.

The electro-chemical properties shew why oxigen produces with some bodies acids, and with others alkalis; and that it ought not to be called the acidifying principle,

The electro-chemical explanation in which the combustible radical of an acid is already in its non-oxided state, electronegative towards the radicals of the salifiable bases, being considered along with the experimental determination, that the radicals of the bases, and of the acids, very often combine in the same proportions in the combustible state, as in the oxided state, proves that it is to the nature of the radicals, and not to oxigen, that we ought to attribute the nature of the product of oxidation. In this manner it is that sulphur and potassium combine for the most part in the same proportions, in the sulphuret of potassium, in the hydrosulphuret of potassium, in the sulphuret of potash, in the hydrosulphuret of potash, in the sulphite, and in the sulphate of potash; and it is by no means difficult to observe, that sulphur not oxided, performs the part of an acid, that is to say, of an electronegative constituent in the sulphuret and the hydrosulphuret of potash. These observations upon sulphur and potassium may be applied to all the other combustible

and from these and other facts, it is shewn, that it depends on the nature of the radical and not upon oxigen, whether the oxide shall be an acid or a base.

bustible radicals which possess an opposite electric nature. And from all this it follows that *it is the radical itself and not the oxigen which determines whether the oxide shall be an acid or a base.*

Whether the electricities and caloric be matter.

A great question still remains to be discussed; *Whether the electricities and caloric be matter or merely phenomena?* This question has long been disputed, and will long continue in dispute before it shall be decided; which, perhaps, will never be done. At present we must content ourselves with reasoning, though our arguments can at best be considered as the sport of imagination upon interesting objects.

Though they do not exhibit gravitation, nor aggregation,

yet they may be matter.

The hypothesis that light is oscillation objectionable; because,

the oscillating matter is unknown;

and mere oscillation will not explain the chemical changes.

If, by the word matter, we understand a body which manifests its presence by gravitation, which possesses a certain kind of aggregation, and fills the place of its existence, in such a manner as to exclude all other bodies—it will certainly follow, that these problematic beings are not matter. But is it not possible that they should be matter, without possessing these characters; or are the reasons greater for considering them as phenomena?—A number of philosophers have considered light as the oscillations in a problematical matter produced by luminous bodies; and this hypothesis owes its origin to the analogy which exists between sound and light. But this oscillating matter has not yet been discovered by chemistry; and consequently the hypothesis itself cannot be satisfactory, because it presupposes a thing of which we cannot find the existence. But if we even admit that light, and the mechanical phenomena which are presented in its motion, can be attributed to a vibration analogous to that which constitutes sound, this mechanical motion cannot produce the chemical effects of light; such as the alterations in the form, the aggregation, or the composition of bodies; more especially as we have never discovered that sound could produce any such effects. There is, consequently, some probability that caloric may be matter, and that light and all radiations may consist in modes of propagating that matter.

Matter may have chemical affinity and not gravitation.

It may be demanded whether we can imagine the existence of a matter possessing chemical affinities without obeying the laws of gravitation. There is certainly no contradiction in this position. We admit the difference between cohesion and gravitation, and we are also led to distinguish the latter from chemical affinity. Some philosophers have sought to prove that caloric possesses weight, though too small to be perceived; but if caloric be even supposed to be matter, it is not probable that it should possess weight, because the property of radiating excludes all the effect of gravitation, and because this matter, if heavy, ought to accumulate without limit in the planetary bodies and at length destroy them.

Caloric does not gravitate.

From the relation which exists between caloric and the electricities, it is clear that what may be true with regard to the materiality of one of them, must also be true with regard to that of the other. There are, however, a quantity of phenomena produced by electricity, which do not admit of explanation, without admitting at the same time that electricity is matter. Electricity, for instance, very often detaches every thing which covers the surface of those bodies which conduct it. It, indeed, passes through conductors without leaving any trace of its passage; but it penetrates non-conductors which oppose its course, and makes a perforation precisely of the same discription as would have been made by some thing which had need of place for its passage. We often observe this when electric jars are broken by an over-charge, or when the electric shock is passed through a number of cards, &c.

If electricity be matter, it is concluded that caloric is also matter:

But electricity detaches the coverings of conductors;

and perforates bodies:

We may, therefore, at least with some probability, imagine caloric and the electricities to be matter destitute of gravitation, but possessing affinity to gravitating bodies. When they are not confined by these affinities, they tend to place themselves in equilibrium in the universe. The suns destroy at every moment this equilibrium, and they send the re-united electricities in the form of luminous rays towards the planetary bodies, upon the surface of which, the rays being arrested, manifest themselves as caloric; and this last in its turn, during the time required to replace it in equilibrio in the universe, supports the chemical activity of organic and inorganic nature. If we can imagine all this to be possible, we possess a notion how the sun can cause a body to emanate from itself without loss of its own volume, and without this emanated body producing on the bodies which arrest it the effects of a gravitating and falling matter.

Whence it appears probable that caloric and the electricities are matter, &c.:

But it is proper to put an end to these conjectures. I hope

Apology for

that

conjectural speculations.

that the necessity of referring to some electro-chemical theory will excuse the attempt of having imagined one; and though this necessity cannot be pleaded in justification of extravagant conjectures, they will, perhaps, be thought pardonable in a department of science where experiment is yet wanting to regulate the efforts of imagination, although such efforts may be useful to arrange the existing facts, and indicate the course which may lead to the discovery of new ones.

(To be continued.)

II.

Notice respecting Experiments on the Freezing of Alcohol, by Mr. HUTTON*.

The author's motives for publication.

I HAVE been prevailed upon to communicate a notice of some experiments and observations I have made on the production of a great degree of cold. It is scarcely necessary to observe, that my doing so at this time is not a matter of choice: these experiments and observations were mentioned to my friends, as they were made without any injunction as to secrecy, as I did not anticipate that such communications would either be received with so much avidity, or repeated with so much eagerness. The consequence has been, that accounts of these experiments have now got into very general circulation, and many very contrary and erroneous ideas have been entertained, not only as to their extent, but even as to their nature; and it has been imagined, that a communication like the present is the only way to obviate these misconceptions —misconceptions which I owe as much to you as to myself to remove.

Advantages to be derived by change of form in bodies from cooling.

The importance of a method of producing a great degree of cold becomes apparent, when it is considered, that it is at present a very common opinion among chemists,—an opinion founded on a very general analogy, that all gases may be reduced to the state of liquids by the abstraction of caloric, and that by a farther abstraction of caloric all liquids in their turn, may be reduced to the solid state. If this be true, and we were in possession of a method of sufficiently abstracting caloric, all bodies whatever might be reduced to the solid state. We should thus become acquainted with a great number of substances that we have hitherto had no opportunity of examining; many powerful agents would likely be obtained; many new and interesting compounds formed, and much light could not fail to be thrown on the constitution of known substances.

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* Read to the Edinburgh Institute, on the 2d of Feb. last.

Mention and date of a method for producing extreme cold.

Directing my attention to this subject, in the summer of 1810, a method occurred to me, by which I imagined a greater degree of cold might be produced than had hitherto been obtained. Although the power of this method appeared in theory almost indefinite, yet it was easy to foresee that in practice many circumstances might at first concur to set limits to its application; from the nature of these circumstances, however, it was to be expected, that some of them might be considerably modified, and many of them might in time be altogether removed; and thus the practice made, in some degree, to approximate to the theory.

At the time this method occurred to me, the pressure of my professional avocations did not allow me to prosecute it; but, as I anticipated some leisure in the following autumn, I immediately began to provide, at any leisure moments I had, such apparatus as I considered absolutely necessary, or was most likely to be useful. The little dependence, however, which is to be placed on general reasoning on such subjects, and the apprehension that the method might have been previously tried, and found insufficient by others, prevented me from providing any very extensive apparatus.

A thermometer of alcohol (sp. gr. 798)

My first experiment was tried in the following autumn. The thermometer was filled and sealed by myself. The tube was previously tried by the common method, and found, as nearly as such tubes are commonly to be met with, of equal calibre throughout. The spirit with which it was filled was prepared by Richter's process, and afterwards re-distilled by itself. Its specific gravity at 62° was 798. The points 60° and 100° were determined by a mercurial thermometer, which had been made with the usual precautions; the interval was divided into four spaces, each of which, of course, correspond to 10°; the part of the stem below 60° measured nearly 18 of these spaces. A mark was made at every space, till, on arriving at the end of the 17th, the graduation could not be carried farther. This point, of course, corresponded to $+ 60^\circ - 170^\circ = - 110$ deg. of Fahrenheit's scale.

was exposed to the cold by this method, and the alcohol partly frozen.

This thermometer was exposed to the cold produced by the method alluded to, and after some time was examined, when the alcohol was found to have passed all the marks, and was obviously sunk within the ball of the thermometer. A slight degree of discoloration was observable. The thermometer was replaced, and examined about five minutes afterwards, when the ball of the thermometer was found broken, and crystals adhered to the fragments.

Other portions of the alcohol completely frozen in a tube.

I next took a glass tube, about 3-10ths of an inch in diameter, and sealed at one end; into this I poured alcohol till it stood in the tube 4-10ths of an inch deep, and then exposed it to the cold, produced as before; after some time it was so completely solid, that on inverting the tube it did not drop, and only a very minute stream was perceived to glide slowly down the inside of the tube; when this stream had reached nearly the middle of the tube, the whole suddenly fell out, and, pitching in a glass, was broken into several pieces, which quickly melted.

This experiment was several times repeated, but by allowing the alcohol to remain a little longer exposed to the cold, it became so completely solid, that on inverting the tube, not the least portion of fluid could be perceived to separate from the mass.

In order to be as certain as possible of the strength of the alcohol I employed, I again took its specific gravity, and the result corresponded with what I before obtained.

These experiments, therefore, left me no room to doubt that I had frozen alcohol, which, at the temperature of 62°, is of the specific gravity 798.

Repetitions of the experiment.

Being appointed to deliver the course of lectures on chemistry for the session 1810-11, I had no leisure, at that time, to pursue these experiments. They were resumed, however, in the autumn of 1811. The second experiment was repeated and varied, and solid masses of alcohol of some magnitude obtained.

tained. Some of these I soldered together, using as a hot bolt, a rod of frozen mercury, and sometimes a straw cooled down to a very low temperature.

It now appeared to me to be an object of some importance to ascertain the form of the crystals which this substance assumes. This I found attended with some difficulties, which I did not anticipate, and attempts to overcome them have led to the discovery of some facts which I did not at all expect.

The alcohol separated into three distinct strata.

The common masses exhibited crystals of different forms; two kinds appeared to predominate, and each was tolerably distinct in its kind; but it was not very easy to perceive by what increments or decrements the one could be supposed to pass into the other; a rather casual circumstance, however, explained the source of this variety. Attempting to freeze alcohol by a modification of the general process, which I conjectured would yield more regular crystals than the common method, I observed, that before crystallizing, the alcohol separated into three very distinct strata; the uppermost was of a pale, yellowish green, while the second was of a very pale yellow colour; both these strata were very thin; the last mentioned was rather the thickest; the lowermost stratum was nearly transparent and colourless, and very greatly exceeded the other two in quantity. After allowing a part of the lower stratum, which I conceived to be the pure alcohol, to freeze, I attempted to pour out the remainder; but was prevented by the upper strata, which proved to be solidified. The lowermost of these two strata bore some marks of crystallization; the upper had none, and proved so firm, as to resist a straw with which I attempted to perforate it, to open a passage for the sublatent liquid. On removing part of these superior strata, and decanting the remaining fluid, the crystals of the lower stratum appeared very distinctly to be rectangular prisms of equal planes, a few of them on one side of the glass surmounted by quadrangular pyramids, but most of them by dihedral summits. This experiment I repeated several times, and the results coincided.

The lower stratum, or greater part, gave rectangular prismatic crystals.

The alcohol, when fused, had suffered no change.

In order to ascertain whether these phenomena arose from a decomposition of the alcohol, or from the separation of foreign substances previously held by it in solution, the products of several of these experiments were mingled together in a stoppered matrass; the whole was then raised to the temperature of about 120 deg. by a water bath of that temperature. The substances forming the different strata united together, and formed a colourless liquor, which had the specific gravity, and all the other properties of the alcohol from which it was obtained. This experiment was repeated several times, and the results were uniform, affording sufficient evidence, that the alcohol had not been decomposed by this process, but that the superior strata consisted of foreign substances, which it had held in solution. The variety in the form of the crystals obtained by former experiments, was, therefore, most likely occasioned by the presence of these foreign substances, a phenomenon not uncommon in chemistry.

The result of these experiments led me now to perceive, that the assumption that alcohol, prepared by Richter's process, is perfectly pure, or at most contains only a very minute portion of water, is entirely gratuitous. The diluted alcohol of commerce, from which the more concentrated is obtained, is well known to contain different volatile impurities; and since Richter's process makes no provision for the separation of these, we ought rather to expect still to meet with some portion of them in alcohol prepared in this manner.

roperties of he three substances.

I next proceeded to examine the properties of the different substances into which I had separated Richter's alcohol; but the time I had now left for this purpose was too short for making much progress in this inquiry; a few only of their habitudes with water, and one with another, were all that I had time to examine; even these I could examine only imperfectly.

The lowermost stratum, or nearly colourless fluid, which I have called alcohol, had no flavour, and produced on the organ of smell only a sharp pungent sensation. It has the remarkable property of smoking when exposed to the air, and when diluted with water it differs considerably in taste from common diluted spirit of wine.

The pale yellow substance, or second stratum, has a pungent taste, leaving an impression of sweetness. It has a very strong but agreeable smell. When mixed with the alcohol, and diluted with water, it has very much the flavour of the better kinds of highland whisky. It readily dissolves in water, and communicates to that fluid its peculiar flavour.

The pale, yellowish green substance, which composes the uppermost stratum, has a strong and very offensive smell, and a very sharp nauseous taste. It dissolves in alcohol, to which it communicates its peculiar flavour; its disagreeable smell is considerably heightened by this combination. It dissolves in water, though less readily than the substance last treated of. The compound, when much diluted and heated, has very much the flavour of the *low wine* of our lowland distillers, at the time it issues from the still.

The two last mentioned substances, or those of which the two upper strata are composed, when mixed together and greatly diluted with water, have very nearly the flavour of alcohol. They have rather more volatility than water; for when half a solution of them has been distilled over, the distilled part has a much stronger smell than that which remains in the retort.

It may be proper to mention, that from the circumstance of my sense of smell having been for some time extremely obtuse, I have been under the necessity of trusting to others for the facts regarding the flavour of these new substances and mixtures; from the uniformity of the reports, however, which I have received from different persons, I have no doubt that these facts are correct.

Alcohols of less strength frozen with similar results.

Besides that from which I filled the thermometer in the first experiment, I have operated on alcohol of the specific gravities 802, 797, and 784; the specific gravity of the last was taken when its temperature was 66 deg. and it is probably the most concentrated that has ever been obtained. But with alcohol of all these different strengths, the general results were similar. In alcohol obtained from different sources, the proportions of the impurities were different, both with regard to the pure alcohol, and to one another, but I have met with none that did not contain both.

From these experiments I think it is ascertained;

Recapitulation.

1st. That the strongest alcohol which we are able to obtain, may be frozen by the method alluded to.

2d. That this alcohol contains at least two foreign substances, which are highly volatile, and, so far as is known, can only be separated by freezing.

3d. That it is to those substances that alcohol owes its peculiar flavour, and that, according as the one or other predominates, the flavour of the alcohol is agreeable, or otherwise.

Last autumn I resumed this subject, and my attention was chiefly directed to the habitudes of these impurities with the chemical re-agents. This I found attended with considerable difficulties, none of the least of which was to procure a sufficient quantity of these impurities in a separate state. The series of experiments I proposed to myself on this subject have not yet been completed; but I may remark, that the result of some of those I have made, promises to afford practical hints of considerable importance to those brewers whose products are intended to afford spirituous liquors.

From this notice it will be observed, that I have scarcely yet entered on the wide field of inquiry, for cultivation of which, the method alluded to appears to offer so powerful an instrument. Alcohol only has been subjected to experiment; it was the only liquid which had resisted all attempts to reduce it to the solid state by the abstraction of caloric. If these experiments be correct, we may now pronounce it a general law, to which there is no exception, that all liquids with which we are acquainted may be reduced to the solid state by a suitable abstraction of caloric. Whether all gases may be susceptible of reduction to the solid state, by abstraction of caloric, remains to be ascertained; although, as I have mentioned, analogy renders it in the highest degree probable.

The examination of the singular substances, which alcohol prepared by Ritcher's process contains, has drawn me aside from the course of experiments I prescribed to myself, and taken up that time which I intended to have devoted to the examination of the effects of cold on the gaseous bodies. Whether I shall proceed to these bodies, or resume the examination of the habitudes of the alcoholic impurities with the re-agents, will much depend on the leisure which I can obtain; but to whichever of them I may direct my attention, I shall not fail to give the earliest information of the result to the Institute.

Annotation.—W. N.

Remark upon As Mr. Hutton's experiments and observations, and perhaps more inventions, announced, but not described.

more or less of his method, were communicated to his friends, it is to be regretted that he has not described it in this notice; which would, at least, have secured him against the pretensions of those who, from conjecture or otherwise, might perform the same. Without departing from the respect due to an inventor, I consider it to be quite allowable for me to make a few remarks in this place, for the gratification of such of my readers as may not be familiar with the general subject.

Except by mere communication, and perhaps combination, there are no cooling processes known, but what arise from fusion or gazification. The *first* is by freezing mixture,

If we except the direct cooling process, by communication with bodies at a lower temperature, and the few instances, if any, wherein cold can be said to be produced by chemical union, without change as to the state of aggregation, we can look to no other means of depressing the temperature of bodies, within our knowledge, but such as may be founded upon their augmentation of capacity for heat, when they pass from the solid to the fluid, or from the fluid to the gaseous state. In *the first* of these two methods, certain bodies, such as snow and salt, one at least being in the solid state, are mixed and combine; and if the combination be not congealable at (or its freezing point be lower than) the heat of the surrounding or neighbouring bodies, the compound will be fluid, and will take from those bodies all that heat which its increased capacity as a fluid demands, for the maintenance of that state; and consequently those bodies will be cooled,—and one limit of this process will be at the freezing point of the compound, below which it cannot go; though from the heat of the surrounding bodies, it may be prevented from arriving at that point.

which in practice can scarcely be limited, but by the surrounding heat.

But many of the freezing mixtures, at present known, seem to have their point of congelation far beneath any temperature we can practically look to; and, therefore, a very considerable part of the process of cooling by means of them has been directed to the prevention of the effect of foreign heat, by first cooling the ingredients, and surrounding the vessels with other cooling materials. Whether these precautions have been as much varied and applied, as the circumstances appear to demand, may, with justice, be doubted.

The *second* method is by evaporation;

In the *second* method, by the evaporation of a fluid, such as water in various economical processes, and alcohol and ether in philosophical experiments, the rapidity with which the gaseous state is assumed, under like circumstances, governs the result; and this rapidity will be prodigiously increased by keeping off the surrounding pressure, as in Professor Leslie's experiment. Whether there be any practical limit of temperature, below which these or all volatile or fluid bodies could be prevented from assuming the gaseous state, is, I think, beyond the reach of our inquiries.

and probably subject only to the same limit.

Freezing processes may be improved by discovery or selection of a freezing mixture, and by absorbing the extraneous heat, either by Walker's or Leslie's method.

These cursory remarks upon cooling processes, may lead us to infer, in the way of conjecture, that Mr. Hutton's process may consist in the discovery or use of one of the most powerful freezing mixtures, and preventing the influence of the surrounding heat by a judicious application of the means similar to those pursued by Walker;—or, much rather, that instead of this last, he may have applied Professor Leslie's process as to the external cooling, by evaporation of ether in vacuo, to a vessel containing his freezing mixture. The apparatus for doing this, or for effecting his purpose otherwise, would demand a display of skill which we may reasonably expect will add to the philosophical reputation of Mr. Hutton.

III.

Some Remarks on the Use of Nitrate of Silver, for the Detection of minute Portions of Arsenic. *By* ALEX. MARCET, *M. D. F. R. S.*[*]

Test fo detecting arsenic; viz. solutions of ammonia, and of nitrate of silver, added by alterations.

IN the interesting account of the poisonous effects of arsenic, presented to the Society by Dr. Roget, and published in the second volume of the Medico-Chirurgical Transactions[†], the author has recommended, for the detection of this poison, a test which I pointed out to him, and which, from a variety of experiments, which we tried together, with a view to ascertain its comparative merits, we were induced to consider as the most effectual of all the tests hitherto used for that purpose. The method consists simply in adding, in succession, to the fluid suspected to contain arsenic, minute quantities of solutions of ammonia and of nitrate of silver; by which means, if the smallest quantity of arsenic be present, a dense yellow precipitate will be produced.

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[*] Read to the Medical and Chirurgical Society of London, in December last, and by them published. It is here inserted, not only on account of its intrinsic value, bnt because it bears reference to Mr. Sylvester's paper in our thirty-third volume.—N.

[†] I take this opportunity of stating, at Dr. Roget's request, that the patient, whose case he there related, completely recovered her health, and has remained well ever since.

Yellow precip. if arsenic be present.

All the particulars respecting this mode of detection having been fully stated by Dr. Roget, with such references to former writers on the subject as the case required, it would be quite superfluous to enter into any further detail on this head. My object in resuming the subject, the practical importance of which need not be pointed out, is to communicate to the Society the result of an inquiry which I have made on the nature of the yellow precipitate, the appearance of which is assumed as denoting the presence of arsenic, and to answer some objections which have been made against this test by Mr. Sylvester, of Derby, in a paper on metallic poisons, recently published in Nicholson's Journal*.

Objections by Mr. Sylvester.

The yellow compound in question has the following properties:

Properties of the yellow precipitate.

If, after being well washed with distilled water, it be suffered to stand for some time in an open vessel, it gradually passes to a brown colour; but it does not, like nitrate of silver, become black on continuing this exposure.

It is readily soluble in dilute nitric acid. It also dissolves on adding an excess of ammonia at the moment of its formation; but after it has been separated and dried, it is no longer sensibly soluble in ammonia.

If a small quantity of this precipitate be exposed to the heat of a lamp on a slip of laminated platina, a white smoke arises from it, and metallic silver remains attached to the platina. The reduction of the silver, in the form of a globule, is still more distinct and striking, if a little carbonaceous matter be mixed with the precipitate, and the blowpipe applied.

When the yellow precipitate, inclosed in a tube, is exposed to the heat of a lamp, the white smoke condenses on the cold part of the tube, in minute octoedral crystals of arsenious acid.

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* Nicholson's Journal for December, 1812. Vol. xxxiii. p. 306.

It is an arsenite of silver.

It appears, therefore, that the precipitate in question is a combination of white arsenic (arsenious acid) and silver, or an arsenite of silver; and it is inferred that its formation, when ammonia and nitrate of silver are added to a mixture containing arsenious acid, is owing to a double elective decomposition of the arsenite of ammonia, by the nitrate of silver, in consequence of which arsenite of silver is formed, and separates as an insoluble precipitate from the nitrate of ammonia which remains in the solution. The addition of ammonia is necessary, because arsenic acid alone cannot decompose nitrate of silver; but in Fowler's solution, in which the arsenic is already combined with an alkali, the decomposition takes place at once, without any addition of ammonia. The fixed alkalies, therefore, can answer a similar purpose; but ammonia has this advantage, that it does not, when added singly, decompose nitrate of silver, a circumstance which, in using the fixed alkalies, might occasion some confusion*.

Use and advantage of the ammonia.

Mr. Sylvester's objection; that muriatic acid would, if present, seize the silver.

With regard to Mr. Sylvester's objection, I shall, previous to my offering any remarks upon it, state it in his own words. "If ever muriatic acid be present," says this gentleman, "the test is then wholly useless, as a muriate of silver will be immediately formed, and the yellow compound, said to be so unequivocal in its indication of arsenic, of course be prevented from appearing."

This danger of ambiguity, however, though applying in some degree to the process in question, and well deserving to be noticed, will be found to have been greatly overrated; and there are such easy and obvious means by which this ambiguity can be entirely removed, that it can make no solid objection to the utility of the test.

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* It is necessary, as Dr. Roget has observed in the paper already quoted, that the quantity of ammonia should not be too large; for in that case the precipitate is redissolved. But, even then, it may be made to reappear, by the addition of nitric acid in sufficient quantity to saturate the alkali. In this case, however, the precipitate is not permanent, owing, I find, to its being soluble in the nitrate of ammonia which is formed in the process. Carbonate of ammonia has also the property of producing and redissolving the precipitate.

The fixed alkalies in excess have not the power of redissolving the precipitate.

Remedy. To add an excess of nitrate of silver; which will throw down the arsenic,

There cannot be the least doubt, as Mr. S. observes, but that whenever nitrate of silver is added to a solution containing muriatic acid, a precipitate of muriate of silver must be the consequence. But if the nitrate of silver be added in excess, the arsenite of silver is also thrown down by the intervention of ammonia, and a mixed precipitate of luna cornea and arsenite of silver is obtained, which partakes more or less of the yellow colour of the latter, according to the proportion of the two salts.

and the arsenite of silver may be taken up by dil. nitric acid, and then precip. yellow by ammonia.

If to this dubious precipitate a few drops of dilute nitric acid be added, the arsenite of silver is instantly dissolved, and the muriate of silver, which is insoluble, immediately resumes its peculiar density and whiteness. If a little ammonia be now added to the clear fluid, the yellow precipitate appears in the most distinct manner, and becomes even more characteristic from a comparison with the white precipitate, the appearance of which differs from this in every respect.

By this method, I believe that every objection to the test will be removed; and in order to anticipate all ambiguity, and to avoid any complication or practical difficulty in its application, I would propose to modify the process in the following manner:

Easy manipulation, for this last process.

To the suspected fluid, previously filtered, add, first, a little dilute nitric acid, and, afterwards, nitrate of silver, till it shall cease to produce any precipitate. The muriatic acid being thus removed, whilst the arsenious acid (if any, and in whatever state,) remains in the fluid, the addition of ammonia will instantly produce the yellow precipitate in its characteristic form. It is hardly necessary to add, that the quantity of ammonia must be sufficient to saturate any excess of nitric acid which the solution may contain.

IV.

METEOROLOGICAL JOURNAL.

		BAROMETER.			THERMOMETER.					
1812.	Wind	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain	
12th Mo.										
Dec. 25	N	30·46	30·40	30·430	35	31	33·0			☾
26	N	30·50	30·40	30·450	37	30	33·5			
27	N	30·52	30·48	30·500	36	29	32·5		1	
28	W	30·52	30·32	30·420	43	32	37·5			
29	W	30·32	30·15	30·235	46	42	44·0			
30	W	30·15	29·92	30·035	50	42	46·0			
31	W	29·81	29·75	29·780	44	40	42·0			
1813.										
1st Mo.										
Jan. 1	W	30·09	29·81	29·950	45	38	41·5			
2	S W	30·26	30·09	30·175	44	36	40·0	6		●
3	W	30·30	30·20	30·280	41	34	37·5			
4	S E	30·30	30·09	30·195	42	34	38·0			
5	S W	30·09	29·80	29·975	44	37	40·5		5	
6	S W	29·77	29·70	29·735	50	40	45·0		9	☽
7	N W	29·70	29·30	29·500	46	40	43·0			
8	N W	29·62	29·30	29·460	48	28	38·0			
9	N W	29·87	29·75	29·810	41	31	36·0	9	9	
10	N W	29·82	29·70	29·760	34	28	31·0			
11	S E	29·80	29·70	29·750	40	26	33·0			
12	S E	29·70	29·61	29,655	34	29	31·5			
13	S E	29·58	29·53	29·555	38	34	36·0	5	0·16	○
14	N E	29·74	29·53	29·635	38	33	35·5			
15	N W	30·00	29·74	29·870	38	28	33·0			
16	E	30·20	30·00	30·100	44	29	36·5			
17	S E	30·20	30·04	30·120	35	28	31·5			
18	S E	30·14	30·04	30·090	31	30	30·5			
19	E	30·26	30·14	30·200	33	31	32·0			
20	N E	30·27	30·26	30·265	34	30	32·0			
21	N E	30·35	30·27	30·310	34	29	31·5			
22	N W	30·50	30·35	30·425	36	23	29·5	15		
		30·52	29·30	30·022	50	23	36·25	0·25	0·51	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

1812. *Twelfth Month.* 25. A very slight fall of snow. 27. A little snow last night. 30. 31. Windy night: small rain at intervals.

1813. *First Month.* 1. Small rain at intervals. 3. Misty morning. 5. Windy. 6. Windy: small rain. 7. Very misty, a. m. dark and cloudy, p. m. About 8, some lightning, which was soon followed by a shower. 9. Hoar frost: at 9, a. m. thick air, with *Cirrostratus* and *Cirrocumulus:* sounds come freely from the city, with the wind at S. S. W. Sleet and rain followed within an hour. 13. Overcast, a. m. thin sleet and rain. 14. Cloudy. 19. A little snow, a. m. 22. Clear, p. m. A fine red burst in the horizon at sun-set.

RESULTS.

Winds variable.

Barometer: greatest observed height, 30·52 in.; least 29·30 in.
Mean of the period 30·022 inches.

Thermometer: greatest height 50°; least 23°.
Mean of the period, 36·°25.

Evaporation 0·35 inches. Rain and snow 0·51 inches.

Plaistow,
First Month, 23, 1813.

L. HOWARD.

V.

On the Explosive Compound of Chlorine and Azote. By Messrs. R. Porrett, *jun.* Wm. Wilson, *and* Rupert Kirk.

To Mr. Nicholson.

SIR,

Information received by the authors of the explosive compound of chlorine and azote.

IN the beginning of December, 1812, we learned from some of the newspapers, and from other sources, that a new explosive compound had been discovered at Cambridge, by Mr. Burton; that it was supposed to be a compound of chlorine and azote; that its explosive properties were of the most terrible kind, and had occasioned a very serious accident to Sir H. Davy, who was examining it; that the contact of oil would cause it to explode; that it was formed by exposing a solution of nitrate of ammonia to chlorine gas; and lastly, that the application of a freezing mixture during its formation was advantageous.

Such is the sum of the information which we then obtained, and which stimulated us to undertake a number of experiments with this compound; we have not since procured any further information respecting it, excepting such as we have derived from our own experiments. We state this, in order that your readers may have the means of distinguishing from among our experiments, those few which are not original.

Experiments.

We shall now proceed to relate our experiments, beginning with those which concern the formation of the compound.

Chlorine gas was received over warm water, in glasses of 16 cub. inches, and thence transferred to small basins of the ammon. solutions.

The mode which we adopted for forming it, was, in every instance, to fill with chlorine gas, over warm water, glass receivers of the capacity of about sixteen cubic inches; and to transfer these into small basins, containing the ammoniacal saline solutions. We soon found that the compound could be formed with solutions of other ammoniacal salts besides the nitrate: those which we have successfully employed for obtaining it, are the following:

Other ammon. salts which form the compound.

Sulphate of ammonia,
Phosphate do.
Muriate do.
Nitrate do.

Oxalate of ammonia,
Muriate of zinc, with excess of ammonia,
Muriate of ammonia, and iron by sublimation.

Those with which we did not succeed in forming it are the undermentioned.

Others which did not.

Carbonate of ammonia,
Triple muriate of platina and ammonia,
Sulphate of copper, with excess of ammonia.

We wished to ascertain whether any other solution containing azote might be substituted for the solution of ammoniacal salt. The solution which we tried with this view was one of nitrate of lead at a minimum, but we could not obtain by its means any of the explosive compound. We have not yet made any other experiments of this nature.

Nor did nitrate of lead, at the minimum.

There are certain bodies which, if present during the process for forming the explosive compound, prevent its formation, or at least prevent it from appearing. Of this description of bodies we have observed the following:

Sulphur, charcoal, carb. acid, atmos. air, and hidrog. gas prevent the compound being formed.

Sulphur, in solution in the ammonia,
Do. in powder within the receiver,
Charcoal in fine powder, adhering to the interior moist surface of the receiver,
Carbonic acid gas, equal in volume to one-third the chlorine gas,
Atmospheric air, do. do.
Hidrogen gas, equal in volume to the chlorine gas.

With respect to the temperature best adapted for the formation of the compound, our experiments lead us to quite an opposite conclusion from what has been published. The employment of a freezing mixture, instead of being advantageous, we have found to be the reverse, as we have never succeeded in obtaining the compound when the solution and the gas were at a temperature below 32°. In these instances, a thin crystalline icy film, was observed to line the sides of that part of the receiver containing the gas, and unless this was dissolved again by raising the temperature, no explosive compound was produced. On the contrary, when we have employed solutions of ammoniacal salts at the temperature of 90°, the explosive compound has been abundantly and quickly formed.

It is not formed at temperatures below the freezing point of water; but best at higher temperatures.

Beautiful effect at 125 deg.

which ceased at 110 deg.

formed. In one experiment we heated the solution to 180°, and observed, in ten minutes after, when about half the gas was absorbed, and the temperature had lowered to 125°, that the receiver above the fluid was covered with the explosive compound, which trickled down to the surface of the solution in minute globules, which converged from all parts of the circumference of the circle forming the surface of the solution to the centre of that circle where they accumulated into larger globules. This phenomenon, which had a very beautiful appearance, seemed to us to be owing to a distillation of the compound from the central or hottest part, and a condensation at the exterior or coldest part of the receiver. This distillation ceased when the temperature had lowered to 110°, and the explosive compound then formed a film on the surface of the solution.

The phenomena attending the formation of the compound, are the following:

Phenomena of the formation of the compound.

Absorption of the gas, film of the compound seen after one-fourth of gas has been absorbed.

Globules of the compound which enlarge and sink.

As soon as the receiver of chlorine gas is placed in the solution of the ammoniacal salt, an absorption of the gas commences, and the solution rises slowly in the receiver. An action is apparent on the surface of the solution, which resembles small filaments reaching to the depth of about one-tenth of an inch. These filaments, on close inspection, appear to be composed of extremely minute bubbles of gas, ranged in a line one above another to the surface. When about one-fourth of the gas has disappeared, some of the explosive compound may generally be observed on the surface of the solution in a thin film; the surface then looks oily, and appears divided, so as to give the idea of a map. As the solution rises in the receiver, the quantity of the explosive compound increases; and it then collects into one or two flattened globules, which when they become very bulky, fall through the solution to the bottom. The whole of the gas is absorbed. The solution, after the formation of the compound, contains free muriatic acid, and also some of the compound in solution, if we may judge from its smell and yellow colour. We are not aware, that there are any other appearances during the formation of the compound, which are material to notice.

Theory.

The following appears, from our experiments, to be the theory of the formation of the explosive compound.

When

Part of the chlorine gas forms mur. acid with the hidrogen of the ammonia, and part forms the compound with its azote. The compound being soluble in the gas, is not visible till much of the gas is absorbed.

When an aqueous solution of muriate of ammonia is brought into contact with pure chlorine gas, one part of the chlorine is dissolved in the solution, and there decomposes the ammonia of the salt, by combining with its hydrogen, (with which it forms muriatic acid,) and sets free its azote, to combine with another part of the chlorine, with which it forms the explosive compound. The compound which is at first formed in this manner, is not visible because it is soluble in chlorine gas, and there is at first an excess of that present; but in proportion as the quantity of this gas diminishes by combining with the elements of the ammonia, the explosive compound appears, and is deposited by the gas, generally on the surface of the solution, but sometimes considerably above it on the upper part of the receiver. The former effect is most likely to take place when the upper part of the receiver is in the form of a dome, or circular; the latter, when it is in that of an inverted cone, or funnel shaped. The relative temperatures of the surface of the solution, and of that of the top of the receiver, have also, as might be expected, a considerable influence in determining where the compound shall be deposited. Its natural situation, from its high specific gravity, is at the bottom of the solution; but unless it is in large quantity, or has been agitated, it remains where it is formed, on the surface of the fluid; preserving that situation by taking a flattened spherical form, like that which a heavy oil assumes on the surface of water.

This explanation applies to other ammoniacal salts.

The explanation above given of the formation of the compound from solution of muriate of ammonia, applies equally when solutions of any other salt, formed of an incombustible acid and of ammonia, are employed; the nature of the incombustible acid (with the exception of the carbonic) being of no importance, the only use of the acid being to prevent, by engaging the ammonia, the rapid action which the chlorine gas would exert on that alkali in an uncombined state: the existence of it in that state would also be incompatible with that of the explosive compound. This last assertion may appear extraordinary to those who know that the explosive compound may be formed by confining chlorine gas over a solution of pure ammonia; but it is nevertheless true; for in this case the explosive compound, although apparently formed from pure ammonia, is, in fact, formed from the muriate of that alkali;

which muriate is one of the products of the exposure of pure ammonia to chlorine gas.

The results differ with the proportions. If the free ammonia be in excess, muriate of ammonia only is formed, and azote set free.

Two different results are obtained from the mutual action of chlorine and ammonia, depending on the proportions of the two bodies presented to each other. Thus, when the quantity of ammonia present in a free state, is more than the chlorine gas can decompose and neutralize, the whole of the chlorine gas goes to the formation of muriate of ammonia, and no explosive compound is formed, but in its stead azotic gas is found at the termination of the experiment, equal in volume to one-third of that of the chlorine gas employed. Thus the only products of the experiment, under these circumstances, are the muriatic acid of the muriate of ammonia, and the azotic gas.

But when the chlorine gas is in excess, the last-mentioned azote is employed in forming the compound.

But when the quantity of chlorine gas present is more than is necessary to bring the ammonia to a neutral state; or, which is still better, when the ammonia has been previously neutralized by an acid, the azote, instead of remaining after the experiment in a state of gas, is found combined with the superabundant chlorine, forming the explosive compound. Thus the products of the experiment, conducted in this way, are, the muriatic acid which remains in the solution, and the explosive compound.

In the case first stated, the chlorine combines with one of the elements of the ammonia only, viz. the hidrogen; in that last described, it combines with both, viz. the hidrogen and the azote.

Experiment on the proportions.

We shall here relate an experiment made with the intention of ascertaining the proportions of chlorine and azotic gases, which, in a condensed state, form the explosive compound.

The compound was decomposed by potash and also by ammonia.

Two globules of the explosive compound produced from equal quantities of chlorine gas, and apparently of the same size, were decomposed; the one by potash dissolved in water, the other by solution of pure ammonia; the gases from each were collected and measured; that from the first was 0·8 of a cubic inch, and that from the last 1·1.

Phosphorus was heated in both; in that produced over the solution of potash it burnt, and caused its volume to diminish to 0·66; in that produced over the solution of ammonia, it did not burn, and caused its volume to increase to 1·3.

Now, if we suppose the two portions of gas, after the action of the phosphorus, to be in the same state, i.e. to be phosphuretted azotic gas, each containing, with respect to their volume, the same proportions of phosphorus, it will not be necessary, for the following calculation, to make any correction for the augmentation in bulk occasioned by the phosphorus; and as the circumstances of temperature and pressure were the same with both, neither will any corrections be necessary for those circumstances—we may, therefore, consider the comparative volumes of azotic gas produced in the two experiments, as represented by 66 and 130, and their difference as 64, being the excess of azotic gas produced over the ammoniacal solution. If we multiply this by 3, (the volume of chlorine gas necessary to produce 1 part of azotic gas from ammonia) we shall have 1·92, which will represent as gas the quantity of chlorine in one of the globules. And the quantity of azote, brought to the state of gas from the other, being, according to the first experiment, 0·66, makes the composition of the explosive compound to be nearly three parts of chlorine gas to one of azotic gas, condensed to a degree which we have not yet estimated.

Deduction that the new compound contained three parts chlorine gas and one azote.

We do not state this analysis as deserving much confidence—it must be frequently repeated before we can put any faith in it ourselves.

Our principal motive in describing the above experiment, before we have had an opportunity of repeating it, is to shew an easy and practicable mode of analysing the compound.

It may be proper now to describe some of the physical properties of the explosive compound.

Physical properties of the explosive compound.

Its colour is that of bees' wax; it is very fluid; it sinks, although with extreme slowness, in a solution of red sulphate of iron of the specific gravity of 1·578. Hence we conclude, that it must be of the specific gravity of about 1·6. It disappears after some time, even under the surface of water, or of the solution in which it was formed; but evaporates almost instantaneously when exposed to the air; it then diffuses its peculiar and penetrating odour through the surrounding atmosphere, which then affects the eyes in a very painful manner, causing them to shed tears. Its action on the lungs, however, we conceive to be much milder and less prejudicial than that of chlorine

Colour like bees' wax; very fluid; sp. grav. 1·6; soon disappears; very evaporable; smell offensive and noxious, but less so than of chlorine.

chlorine gas, as we have experienced very little inconvenience in this respect from standing close to a solution, from the surface of which the compound was diffusing itself into the atmosphere.

Very volatile, but may be kept in a close vessel.

The volatility of the compound is so great, as to present a considerable obstacle to preserving it; we have, however, found, that by limiting the quantity of air or of fluid which can come into contact with it, and at the same time preventing the escape of vapour by pressure, it can be kept for any length of time. We have accomplished this by introducing the compound into small tubes, closed at one end, about nine inches long, being first filled with some of the solution. The compound should occupy at least half an inch from the bottom of the tube, and some of the solution should afterwards be taken out to leave room for a little air, and to allow of the open end of the tube being hermetically sealed before the blowpipe. When any of these tubes are afterwards broken, the escape of compressed vapour is so considerable, as to occasion a loud report.

Difficult to transfer, because so volatile.

In our first experiments with the explosive compound, we experienced considerable difficulties in transferring it from one vessel to another, as we had no better mode than that of introducing into the solution and under the compound, a small spoon of tinned iron; the motion which this communicated to the compound often carried it to the surface, where it extended itself and disappeared, by dissolving in the atmosphere. In order to remedy this, and other inconveniences attending on this method, we invented a little instrument which we have found to answer our most sanguine expectations; it is formed of a small glass tube, of the size of a large writing quill, open at one end, and closed at the other, in the manner of a test tube, with the exception of a small circular hole in the centre. This tube is to be used as a syringe, the piston of which is to be formed of cotton, wound round a piston rod of wood or copper; by raising or depressing which, the explosive compound may be drawn into, or ejected from, the tube with the greatest facility. The peculiar advantages of this instrument are, its taking up the compound with so small a quantity of the solution, and with so much celerity, and its retaining it when he

A small instrument or glass syringe for taking up the compound.

tube is held in an inclined position, owing to the concave bed in which the compound lies; in short, this instrument combines the advantages of a spoon, with that of a common syringe. It is represented with a globule of the compound in it in the following outline—fig. 3, Plate V, where *a* is the glass tube, *b* the circular orifice, *c* the piston and rod, *d* a globule of the compound.

Precautions against explosion.

A precaution very necessary to be taken in the use of this instrument is, that it be clean, or at least free from oil, grease, or any combustible matter, which might, by causing the compound to explode, occasion a serious accident. This precaution is also very necessary with respect to all other vessels with which the compound may come into contact. Another general precaution, which we strongly recommend to those who may make experiments with this compound, is, to wear a mask on the face, and gloves on the hands. We conceive it also very proper to state, that although the results of upwards of two hundred different experiments which we have made with this compound are in favour of the conclusion, that it will not explode without the contact of a combustible body, or the application of a temperature exceeding 200°; yet three explosions have taken place, the causes of which remain unknown to us, as we were not aware of the compound being in contact with any other body than cold water. These explosions were, therefore, completely unexpected by us; but fortunately, they did not occasion any accidents of a serious nature.

Effects of different temperatures on the compound.

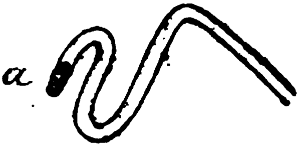
The effects of different temperatures on the compound we considered as very deserving of investigation, for which reason we made the following experiments:

It was not frozen at —16 deg.

A globule of the explosive compound was introduced into a small tube filled with water, it immediately fell to the bottom. The tube, with its contents, was then placed into a mixture of snow and nitric acid, into which a thermometer was also placed. The mercury fell to — 16°; the water in the tube was of course solidified, but the compound retained its fluidity, and was not altered in any respect.

At 160 deg. it came over by distillation.

A globule of the compound was introduced into a tube, closed at one end, of the form represented in fig. 4, pl. V: it was previously filled with the solution from which the compound was formed.



The globule is represented at *a*; the bent part of the tube was then placed in a vessel of water, and its open end immersed into a wider tube, also filled with some of the same solution. The vessel containing the water was then heated. When the temperature approached to 160°, it began to distil; at 160° the distillation was rapid, the compound being converted to vapour in the bent tube, which served as a retort, and was condensed and collected in that which was disposed as a receiver—much gas was given out during the process. The double curvature of the retort tube was to prevent any of the compound being floated over by bubbles of gas attaching themselves to it. This precaution we found was very necessary.

It did not explode at 200 deg.

A globule of the compound covered with water, contained in a little spoon of tinned iron, was with the spoon introduced into a quantity of water heated to 200°—this temperature was not sufficient to make it explode; it merely occasioned its vaporization.

But it did violently at 212.

The last experiment was repeated, varying only the temperature of the water, which, in this instance, was 212°; the compound immediately exploded violently.

These experiments prove, that the explosive compound does not assume the solid form at — 16°, that it may be distilled at or below 160°, and does not explode but at a temperature above 200°, suddenly applied.

Our next object was to ascertain whether, when the natural pressure of the atmosphere was taken off, or diminished, from the explosive compound, it would still retain the fluid form, or whether it would assume the elastic state—with this view we made the following experiment:

Apparatus for exposing

A tube, 31 inches long, closed at the bottom, had another tube of smaller bore, but of the same length, and open at both ends, the compound in a vacuum.

ends, placed within it; both tubes were then filled with mercury, excepting about one-fourth of an inch at the top. This one-fourth of an inch was afterwards filled with the following—

1st. A small glass cup containing the explosive compound covered with a drop of muriate of lime. This cup moved freely within the tube.

2d. Muriate of lime in solution, surrounding and rising above the glass cup.

3d. A glass stopper, ground to the tube, and closing it accurately.

The compound, when in vacuo, assumed the state of vapour.

The inner tube was then raised thirty inches above the level of the mercury in the outer one. The column of mercury in the inner tube descended seven inches, leaving a column of twenty-three inches only. These seven inches were occupied by the explosive compound in a state of vapour: but as a little of it still remained in the cup, not converted to that state, a temperature of about 100° was applied to it—this caused it to disappear, and the vapour, after cooling, then occupied another inch, the mercurial column being reduced to twenty-two inches. The tube being now lowered until the mercury within and without it were of the same level, the explosive compound reappeared. There remained, however, seven-eighths of an inch of permanent gas, which an accident prevented us from examining; but we are inclined to believe, that this small quantity was produced when, on lowering the tube, the mercury rose into that part which had been occupied by the vapour, and the sides of which had been wetted with the liquid muriate of lime, which, notwithstanding that it was very concentrated, had probably absorbed some of the vapour, as we observed some bubbles of gas rising through the mercury from that portion of the metal which was in contact with the humid surface.

Experiment of exploding the vapour of the compound.

We repeated the above experiment in the hope, that by applying heat sufficient to make the vapour explode, we might, by this means, analyse the compound. We, therefore, exploded the vapour by surrounding the glass tube with part of a gun barrel heated nearly to redness; but at the instant of the explosion the tube was shattered. We, however, propose to repeat this experiment with a tube of greater strength

We

VII.

Description of a simple Apparatus for Distillation. By a Correspondent

To Mr. Nicholson.

SIR,

THE number of ingenious and beautiful apparatuses for distillation, and the experiments of pneumatic chemistry, give a splendour to the exhibitions of lectures, and are highly gratifying to the affluent cultivators of science. But the greater part of operative chemists every day feel the expence which, from its own brittle nature, and the heavy duties imposed upon it, attends the use of glass. To them the simplicity and cheapness of a set of vessels stand among its most desirable properties. I send you a sketch of a combination which has not, I believe, the recommendation of novelty, but which, from repeated and habitual use, I have found of such value as leads me to believe you will be disposed to bring it into more public notice. It consists simply of A, a retort fitted into B at the neck E, which may be considered as the only indispensable ground joint. Into the other neck of B the vessel C is fitted like the upper vessel of Nooth's apparatus, having its neck D closed by a conical stopper, or, if preferred, a tube of safety may be placed there. In the operation, the distilled matter, or gas, passes over, and is received, condensed, or absorbed, in B. If the pressure be considerable, part of the liquid in B will rise into C, the included air of which last vessel may raise the stopper D, and partly escape.

If there be reason, from the nature of the subject, to apprehend, that part of the contents of the retort may boil over; or if the first products of distillation be required not to pass into B, the common adopter may be used, as shown by the dotted lines F. G. H. I.

I am,
Sir,
Your Constant Reader,
A.

VIII.

VIII.

Upon certain ready Processses for Computation, supposed to have been invented by the American Boy exhibited in London.*

SIR,

I SHALL make no apology for troubling you on a subject, which, though generally esteemed dry and abstruse, has at present acquired, from particular circumstances, considerable interest. There is a boy in town, who is exhibited as a curiosity, from the facility with which he performs several difficult arithmetical operations. It is pretended that this is a gift, and that he has had no instructions to enable him to do this. Now, Sir, as there are easy methods of solving these questions, which are not, I believe, generally known, I shall simply state them to the public, that this matter may, if necessary, be further investigated; and that this boy may be reduced to what he really is—a very clever boy, but no prodigy.

Introductory statement respecting the boy exhibited in London.

In extracting the cube root where it consists of three figures, it is well known that the first figure of the root may be obtained by a simple inspection of the number of millions, and the last figure, by observing the final figure of the number whose root is proposed to be extracted; if then, the middle figure could be found, we should have the root. To find this, square the final figure of the root so previously obtained; multiply this square by 3, call A the last figure of this product. Now cube the last figure of the root, substract its penultimate digit from the penultimate digit of the number given, (adding ten to this last, if it be the smaller of the two) call the result B.

In extracting the cube root of three periods, the first period determines the first figure, and the last digit determines the last figure, and the middle figure is found by a simple process.

Then that number, which being multiplied into A, produces a number terminating with the figure B, is the middle figure of the root. An example or two will make it manifest: suppose 377,933,067 to be proposed; here 7 is the first figure, (as $7^3=343$, the nearest cube below 377) and 3 is the last figure; since the cube of 3 terminates with 7, the last figure of the number. Now to find the middle figure $3^2\times 3=27$.. $A=7$, and $3^3=27$, of which the penultimate figure is 5.

Example of the extraction

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* From the Morning Chronicle of Feb. 17, last. For some account of Zerah Colburn, See our present vol. page 5.

Now the penultimate figure of the number is 6....6−2=4=B. And since 2·7 (or A)=14, the last figure of which is 4 or B. The middle figure of root is 2, and root is 723.

Ambiguity of the rule.

This rule, I should add, becomes ambiguous in all cases where the number proposed terminates with an even digit, or with a 5; thus, in 41,421,736 A=8 and B=2.

Now, as either 4×8=32 or 9×8=72, it follows that, according to the rule, either 4 or 9 might be the middle figure, and either 346 or 396 the root; but as $\overline{396}|^3$=nearly $\overline{400}|^3$, or 64 millions, it appears on inspection of the number proposed, that 346 must be the true answer. No error would, therefore, be produced by this ambiguity. Indeed, the only cases of ambiguity which can deceive, are in numbers terminating with 5.

The rule for extracting the square root is not much different

—— but the ambiguity is greater.

The rule for the square root differs only in these particulars; to determine A, take the simple power of the last figure of the root, and instead of 3, multiply by 2. To determine B. subtract the penultimate figure of the square instead of the cube of the last figure of the root. In all other respects, the two rules exactly agree. In the case of square, there is, however, an ambiguity which does not exist in the cube. It happens, that the final figure of a square number gives two figures which may terminate the root; as for instance, $4^2=16$ and $6^2=36$. If, therefore, a square number terminate with 6, its root may terminate with either 4 or 6, and, therefore, more mistakes will occur in the application of the rule. I believe this coincides with the fact; since the boy makes many more errors in the extraction of the square, than in that of the cube root.

Reference to a publication of Rallier des Ourmes, published 45 years ago, and containing the principles of these methods.

The principles of these rules, and the rules themselves, or a very slight modification of them, have been known so long ago as the year 1768; in that year, M. Rallier des Ourmes published two memoirs on the subject. They are to be found in Pp. 485 and 550 of the fifth volume of "Sçavans Etrangers." They are entitled "*Methode Nouvelle, &c.* or *a New Method of dividing, when the dividend is a multipile of the divisor, and of extracting the roots of perfect powers.* See page 550. His method only takes the last figures into account. In the extraction of the higher powers, this is undoubtedly the easier way. The second is, "Methode facile, &c. or *an easy Method of discovering all the*

"*prime numbers contained in an unlimited series of odd numbers*
"*in succession, and at the same time, the simple divisors of*
"*those which are not primes.* This latter memoir is probably the method pursued by the boy to find prime numbers, and to resolve numbers into their factors. Of the method of M. Rallier, he himself says, "*In a word, we do not hesitate to*
"*assert from experiment, that by this method, in a single day,*
"*and in the way of amusement, computations may be effected,*
"*which by the old methods, would require months of severe*
"*labour.*" I will only now add this observation. As the above rules depend upon the two or three first, and the two last figures of any number, it follows that the change of the intermediate ones cannot affect the result. If it should have occured to any one, as it has to me, *to have altered any of these*, and yet to have obtained the true result; it will, I think, not be unfair to conclude, that either of these very methods, or some similar to them in principle, are those adopted. Let me add, that I have no doubt, but that any clever boy would, in a week's time, learn to apply those given above with the utmost facility.

——which are considered to be the same as those practised by the boy, and of easy acquisition by any boy of talent.

I am, Your's, &c.

A. H. E.

[The following is from the same respectable daily Journal of the 18th.]

SIR,

Remedy for the ambiguity.

I agree with your correspondent A. H. E. that the young American is a very clever boy, but no prodigy, as one visit to him has convinced me. The ambiguity of the cases A. H. E. mentions, in extracting the cube root, may be readily cleared by any one conversant in figures in a few seconds, by finding B in the common formula for the cube root, which is the cube of the binomial $A+B$; namely, A^3+3A^2B, &c. which is, no doubt, perfectly well known to A. H. E. though to some of your readers, who may be interested in this matter, it may not be so familiar. For such the following directions may be useful. The first fig. of the root being known by inspection, take its cube from the millions given, then the remainder being divided by the first two digits (for they will be sufficient) of thrice the square of the said first figure, will immediately shew which of the ambiguous figures should be taken for the second figure of the root. Thus, if the proposed number be 465,484,375, here the first and last digits of the root are 7 and 5: A=5 and B=5; any odd number, therefore, multiplied by A will give B; but if the cube of 7=343 be taken from 465, and the remainder 122 be divided by 14 (the first two digits of $7^2 \times 3$) it will be instantly seen that 9 is too great, and 5 is manifestly too little; there only remains 7, therefore, for the second digit of the root. The same method will easily clear the ambiguity when the proposed cube ends with an even digit.

I am, &c.

O.

IX.

On the Appearance and Disappearance of the Aurora Borealis.

By M. COTTE*.

Supposed connection between the aurora borealis and magnetism.

WHETHER there be any relation or agreement between the progressive changes of magnetical variation in a given latitude, and the times at which the aurora borealis appears, or ceases to manifest itself, is a question entitled to discussion. It is proved by observation,

Facts.

1. That the declination of the magnetic needle is not constant; that in our latitudes it was easterly before the year one thousand six hundred and sixty-six; and since that time it has more or less slowly increased to the west.

2. That the phenomenon of the aurora borealis, of which the western part of the atmosphere is also the seat, is seen frequently during certain epochas, and very seldom during others.

Agitation of the needle.

3. That when this phenomenon appears, it sometimes has an influence on the magnetic needle, so as to produce an irregularity of motion, or unsteadiness in the variation of the needle. The same thing sometimes happens in stormy weather, or when much electricity predominates in the atmosphere.

It must be remarked, that this influence of the aurora borealis upon the magnetic needle, does not constantly attend that phenomenon, and that a very feeble aurora borealis has sometimes a more marked influence upon the magnetic needle, than a very brilliant aurora borealis. It likewise happens not unfrequently, that the latter produces no sensible effect upon the magnetic needle. Upon the preceding facts, besides the question first above stated, I would propose the following:

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* Journal de Physique, LXXIII.

Whether the seat of the aurora borealis, in our latitude before the year sixteen hundred and sixty-six, when the magnetic variation was easterly, was likewise in the eastern part of the atmosphere. And whether the times, when the variation is stationary, concur with the times of the disappearance of the aurora borealis; and those in which the variation of the needle is most rapidly changed, concur with the times of the most frequent appearance of the aurora borealis.

The want of accurate observations, before sixteen hundred and sixty-six, in both respects, renders the second question insoluble.

With regard to the first and the third question, the following table affords an outline of the observations which have been made upon the progress of the western declination of the needle since the year sixteen hundred and sixty-six, and the greater or less frequency of the appearance of the aurora borealis, for periods of ten years each.

Minutes of the distinct periods of appearance and disappearance of the aurora borealis.

			°	′		Times.
From	1666 to 1680	increase	1	30	the aurora borealis	7
——	1680 to 1689	——	5	20	——	13
——	1689 to 1700	——	2	12	——	22
——	1700 to 1710	——	2	38	——	59
——	1710 to 1720	——	2	10	——	112
——	1720 to 1724	stationary			——	531
——	1724 to 1730	increase	1	25		
——	1730 to 1740	——	1	5	——	349
——	1740 to 1750	——	1	45	——	84
——	1750 to 1760	——	1	15	no observations	——
——	1760 to 1770	——	1	25		
——	1770 to 1780	——	0	50	the aurora borealis	409
——	1780 to 1790	——	1	1	——	69
——	1790 to 1800	——	0	26	disappearance nearly total.	
——	1800 to 1809	diminish	0	12		

After submitting these observations, I shall only remark, that the nearly total disappearance of the phenomenon of the aurora borealis, which has taken place from the year seventeen hundred and ninety, to the present time, agrees with the diminution of the westerly variation of the magnetic needle, which likewise commenced nearly at the same time.

The observations contained in this notice, may be considered as the commencement of a series in which those afforded by future observers, will, no doubt, be more accurate and extended than what our predecessors have left us.

X.

Description of a portable Instrument for ascertaining the quantity of Grain by weight, called the Chondrometer[*].

The chondrometer consists of a small measure for corn, &c.

IN plate IV. Fig. 2. *A B C* represents a lever or balance moveable on the fulcrum *B* and supported by the stand *G*. The bucket *F* which in the instrument before me has the capacity of $8\frac{3}{4}$ cubic inches, is to be filled with grain, and when taken off and the handle turned back, may have its contents regulated by striking over the surface with a cylindrical straight piece, of about one-seventh of the diameter of the measure. The arm *B C*, carries a division, by means of which the sliding weight *E* can be set to counterpoise the bucket, and its contents at any weight of the latter between twenty-five and seventy-five pounds.

Method of measuring,

It scarcely need be observed, that the quality or product of any kind of grain or flour will, under like circumstances, be better the heavier its weight, and that a portable instrument to ascertain this must afford more accuracy than examination by hand. In the use of the present instrument very little instruction is necessary. The measure is to be filled in the same careful manner as a real bushel, and struck even by the rule, and not by a flat thin edge, which last would carry off too much of the grain; and rough grain such as oats or barley, should be charged a little heavier, because the proportion of these grains torn up by the striking in so small a vessel exceeds what happens in those of larger capacity. The charged measure is then to be hung in its place, and the weight being slided to the proper situation for making a fair counterpoise will indicate on the scale the number of pounds in each bushel of eight gallons.

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[*] The instrument, from which the drawing in the plate was taken, was made by Messrs. Page and Ovenden, in the Strand.

and weighing.

The weights per bushel of the nine following specimens of grain, as stated by Messrs. Payne and Ovenden, are as under :

Weights per bushel of several descriptions of grain.

		lbs.	lbs.		lbs.
Wheat	from	55	to 63	per bushel mean weight,	59
Rye	—	50	— 56	————————	53
Barley	—	45	— 49	————————	47
Oats	—	35	— 42	————————	38½
Pease	—	62	— 67	————————	64½
Small beans	—	60	— 66	————————	63
Dutch clover	—	65	— 71	————————	68
Canary	—	54	— 56	————————	55
Rape	—	47	— 50	————————	48½

XI.

Further Experiments and Observations on the influence of the Brain on the generation of Animal Heat. By B. C. BRODIE, *F. R. S.*

IN the Croonian Lecture for the year 1810*, I give an account of some experiments, which led me to conclude that the production of animal heat is very much under the influence of the nervous system. Some circumstances, which I have since met with, illustrate this subject, and seem to confirm the truth of my former conclusions.

Former experiments.

In an animal, which is under the influence of a poison, that operates by disturbing the functions of the brain, in proportion as the sensibility becomes impaired, so is the power of generating heat impaired also.

Disturbance of the brain impairs the animal heat,

If an animal is apparently dead from a poison of this description,

which is not restored by respiration,

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* Philos. Trans. 1811, or Philos. Journal XXIX. 359.

scription, and the circulation of the blood is afterwards maintained by means of artificial respiration, the generation of heat is found to be as completely destroyed, as if the head had been actually removed.

until the brain recovers.

Under these circumstances, if the artificial respiration is kept up until the effects of the poison cease, as the animal recovers his sensibility, so does he also recover the power of generating heat; but it is not till the nervous energy is completely restored, that heat is produced in sufficient quantity to counteract the cold of the surrounding atmosphere[*].

In such circumstances the chemical changes from artificial respiration were as usual.

In the experiments formerly detailed, as well as in those just mentioned, I observed that the blood underwent the usual alteration of colour in the two systems of capillary vessels, while carbonic acid was evolved from the lungs at each expiration; and hence I was lead to believe, that the respiratory function was performed nearly as under ordinary circumstances, and that the usual chemical changes were produced on the blood. It appeared, however, desirable to obtain some more accurate knowledge on this point, and I have, therefore, instituted a series of experiments, for the purpose of ascertaining the relative quantities of air consumed in breathing, by animals in a natural state, and by animals in which the brain has ceased to perform its office, and I now have the honour of communicating an account of these experiments to this society.

It has been shewn, by Messrs. Allen and Pepys, first,[†] that every cubic inch of carbonic acid requires exactly a cubic inch of oxygen gas for its formation; secondly,[‡] that when respiration is performed by a warm-blooded animal in atmospheric air, the azote remains unaltered, and the carbonic acid exactly equals, volume for volume, the oxygen gas, which disappears.

The watery vapour which escapes in respiration, is not formed during the process,

There is, therefore, reason to believe, that the watery vapour which escapes with the air in expiration, is not formed from the union of hydrogen with oxygen in the lungs, but that it is exhaled from the mucous membrane of the mouth and pharynx, resembling the watery exhalation which takes place from the peritonæum, or any other internal surface when exposed; and this conclusion appears to be fully confirmed by the experiments of M. Magendie, lately communicated to the National Institute of Paris.

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[*] The poison employed in this experiment should be the essential oil of almonds, or some other, the effects of which speedily subside. If the woorara is employed, so long a time elapses before the poison ceases to exert its influence, that it becomes necessary that the experiment should be made in a high temperature, otherwise the great loss of heat which takes place, is sufficient to prevent recovery.

[†] Phil. Trans. 1807, Part II.

[‡] Phil. Trans. 1808, Part II. Ibid. 1809, Part II.

whence the changes in the air from respiration, may be deduced from the carbonic acid given out.

These circumstances are of importance in the present communication, which they render more simple, as they show, that in order to ascertain the changes produced on the air in respiration, it is only necessary to find the quantity of carbonic acid given out from the lungs. This becomes an exact measure of the oxygen consumed, and the azote of the air and the watery vapour expired, need not be taken into the account.

For the purpose of examining the changes produced on the air, by animals breathing under the different circumstances abovementioned, I contrived the apparatus, which is represented in the annexed Plate. Plate VI. Fig. 1.

Description of the Apparatus.

Apparatus.

A. Is a wooden stand in which is a circular groove $\frac{3}{4}$ of an inch in depth, and the same in width.

B. Is a bell-glass, the rim of which is received in the circular groove of the wooden stand. In the upper part of the bell-glass is an opening, admitting a tube connected with the bladder C.

D. Is a bottle of elastic gum, having a brass stop-cock E connected with it.

F. Is a silver tube, of which one end is adapted to receive the tube of the stop-cock E, while the other extremity, making a right angle with the rest of the tube, passes through a hole in the wooden stand, and projects into the cavity of the bell-glass, where it makes a second turn, also at a right angle, and becomes of a smaller diameter. In the upright part of the tube is an opening G.

The tubes are made perfectly air-tight, where connected with each other, and with the rest of the apparatus, and the circular groove is filled with quicksilver.

The capacity of the bell-glass, allowance being made for the rim, which is received in the groove with the quicksilver, is found to be 502 cubic inches. The capacity of the gum-bottle is 52 cubic inches, and in the calculations after the experiments, two cubic inches have been allowed for the air contained in the different tubes, and for the small remains of air in the bladder after being nearly emptied by pressure.

Mode of using the Apparatus.

Use of the apparatus in which an animal was inclosed, and the air afterwards examined.

In order to ascertain the quantity of air consumed under ordinary circumstances, the animal was placed on the stand under the bell-glass, the bladder being emptied by pressure, and the gum-bottle being distended with atmospheric air. During the experiment, by pressing occasionally on the gum-bottle, the air was forced from it into the bell-glass. On removing the pressure, the gum-bottle became filled by its own elasticity with air from the bell-glass. Thus the air was kept in a state of agitation, and the dilatation of the bladder prevented the air being forced through the quicksilver under the edge of the bell-glass. At the end of the experiment, the gum-bottle was completely emptied by pressure, and allowed to be again filled with air from the bell-glass: this was repeated two or three times, and the air in the bottle was then preserved for examination. The proportion of carbonic acid being ascertained, and the capacities of the different parts of the apparatus, and the space occupied by the animal being known, the total quantity of carbonic acid formed, and consequently of oxigen consumed, was easily estimated.

Artificial respiration conducted after the functions of the brain were destroyed.

When the experiment was made on an animal in whom the functions of the brain were destroyed, and in whom, therefore, voluntary respiration had ceased, the narrow extremity of the tube was inserted into an artificial opening in the trachea, and the animal being placed under the bell-glass, the lungs were inflated at regular intervals, by means of pressure made on the gum-bottle. The tube being smaller than the trachea, the greater portion of the air in expiration escaped by the side of the tube into the general cavity of the bell-glass, while the gum-bottle filled itself by its own elasticity with air through the opening G. At the end of the experiment, a portion of air was preserved for examination, and the quantity of carbonic acid was estimated in the way already described.

The air examined by a solution of potash over mercury.

The animals employed in these experiments were of the same species, and nearly of the same size. Attention to these circumstances was judged necessary, that the results might be as conclusive as possible. The chemical examination of the air was made by agitating it in a graduated measure over quicksilver, with a watery solution of potash. My friend, Mr. Brande, gave me his assistance in this part of the present investigation, as he had done on many former occasions. It will be observed, that in estimating the proportion of carbonic acid, no allowance has been made for that contained in the atmosphoric air; first, because the quantity is so small that the omission can occasion no material error; and secondly, because the object is to ascertain, not so much the absolute, as the relative, quantities of carbonic acid evolved by animals breathing under different circumstances.

The experiments which I shall first notice, were made on the respiration of animals in a natural state.

Experiment 1. A rabbit by respiration for thirty miminutes consumed about 50½ cubic inches of oxigen per hour.

Experiment 1. Thermometer 65°, barometer not noted.

A young rabbit was allowed to remain under the bell-glass during thirty minutes. The respired air at the end of this time was found to contain $\frac{1}{20}$ of carbonic acid.

It was ascertained, that the rabbit occupied the space of 50 cubic inches.

The capacity of the bell-glass = 502 cubic inches.

That of the gum bottle 52 cubic inches.

The air in the tubes and bladder = two cubic inches.

$$\text{Then } \frac{502 + 52 + 2 - 50}{20} = \frac{506}{20} = 25{\cdot}3.$$

The rabbit, therefore, in thirty minutes gave out 25·3 cubic inches of carbonic acid, and consumed the same quantity of oxigen gas, which is at the rate of 50·6 in an hour.

Exp. 2. Another at the rate of 56 cubic inches.

Exp. 2. Thermometer 65°, barometer 30·1 inches.

A somewhat smaller rabbit was allowed to remain under the bell-glass during 30 minutes. The respired air contained $\frac{1}{18}$ of carbonic acid. The animal occupied the space of 48 cubic inches.

$$\frac{502 + 52 + 2 - 48}{18} = \frac{508}{18} = 28{\cdot}22.$$

The carbonic acid evolved was, therefore, equal to 28·22 cubic inches in half an hour, which is at the rate of 56·44 cubic inches in an hour.

Exp. 3. Another consumed the same quantity.

Exp. 3. Thermometer 64°, barometer 30·2 inches.

A young rabbit, occupying the space of 48 cubic inches, was allowed to remain under the bell-glass during the same period as in the two former instances. The respired air contained $\frac{1}{18}$ of carbonic acid.

$$\frac{502 + 52 + 2 - 48}{18} = \frac{508}{18} = 28{\cdot}22.$$

The results of this were, therefore, precisely the same as those of the last experiment.

These experiments were made with great care. The animals did not appear to suffer any inconvenience from their confinement, and their temperature was unaltered.

The next order of experiments were made for the purpose of ascertaining the quantity of air consumed by animals, in which the circulation of the blood was kept up by means of artificial respiration, after the brain had ceased to perform its functions.

Exp. 4. Two rabbits were killed by dividing the spinal marrow. The heat fell off but the effect of artificial respiration on the air was not materially different.

Exp 4. Thermometer 65°, barometer not noted.

Having procured two rabbits of the same size and colour, I divided the spinal marrow in the upper part of the neck of one of them. An opening was made in the trachea, and the lungs were inflated at first by means of a small pair of bellows. Two ligatures were passed round the neck, one in the upper, and the other in the lower part behind the trachea. The ligatures were drawn tight, including every thing but the trachea; and the nerves, vessels, and other soft parts between them were divided with a bistoury. Eight minutes after the division of the spinal marrow, the thermometer in the rectum had sunk to 97 The animal was placed under a bell-glass, and the lungs were inflated by pressing on the gum-bottle about fifty times in a minute. When this process had been continued for thirty minutes, a portion of air was preserved for examination. The heart was found acting regularly, but slowly, the thermometer in the rectum had fallen to 90°.

The second rabbit was killed by dividing the spinal marrow about the same time when the experiment was begun on the first rabbit. Being in the same temperature, the time was noted when the thermometer in the rectum had fallen to 97°, and it was placed under another bell-glass, that it might be as nearly as possible under the same circumstances with the first rabbit. At the end of 30 minutes, the thermometer in the rectum had fallen from 97 to 91*.

The air respired by the first rabbit contained $\frac{1}{25}$ of carbonic acid. The bulk of the rabbit was found = 50 cubic inches.

$$\frac{502 + 52 + 2 - 50}{25} = \frac{506}{25} = 20.24.$$

20·24 cubic inches of carbonic acid were, therefore, extricated in 30 minutes, which is at the rate of 40·48 cubic inches in an hour.

The carbonic acid given out in the same space of time was less than in the former experiments; but it is to be observed, first, that in consequence of the ligatures the extent of the circulation was diminished; secondly, that in this instance one of the ligatures accidentally slipped, and an ounce of blood was lost in the beginning of the experiment.

As it was desirable to avoid any circumstances, which might occasion a difference in the results, in the subsequent experiments I employed animals, which had been inoculated with the poison of woorara, or the essential oil of almonds; by which means, while the functions of the brain were completely destroyed, the extent of the circulation was undiminished, and all chance of accidental hæmorrhage was avoided.

Exp. 5. Thermometer 65°, barometer 29·8 inches

Two rabbits were procured, each occupying the space of 45 cubic inches. They were both inoculated with the woorara poison.

The first rabbit was apparently dead in nine minutes after the application of the poison; but the heart continued to act. The lungs were inflated for about two minutes, by means of a pair of bellows, when the thermometer in the rectum was

Exp. 5. Two other rabbits were killed with woorara. The consumption of oxigen, by artificial respiration, in one of them, was not much less than before; but in the

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* In measuring the heat of the rectum in these experiments, care is necessary that the thermometer should always be introduced to exactly the same distance from the external parts, otherwise no positive conclusion can be drawn relative to the loss of heat, as the more internal parts retain their heat longer than the superficial.

observed

other, which was not made to respire, the cooling was rather less rapid.

observed to stand at 98°. The animal was placed under the bell-glass, and artificial respiration was produced by means of pressure on the gum-bottle, as in the last experiment. At the end of 30 minutes, a portion of air was preserved for examination. The thermometer in the rectum had fallen to 91°. the heart still acted with regularity and strength.

The second rabbit died in a few minutes after the inoculation. The time was noted when the thermometer in the rectum had fallen to 96°, and he was placed under a bell-glass. At the end of 30 minutes, the thermometer in the rectum had fallen to 92°.

The air respired by the first rabbit contained $\frac{1}{20}$ of carbonic acid.

$$\frac{502 + 52 + 2 - 45}{20} = \frac{511}{20} = 25{\cdot}55 \text{ cubic inches of carbonic}$$

acid evolved in 30 minutes, which is at the rate of 51·1 cubic inches in an hour.

Exp. 6. Repetition with the like effect.

Exp. 6. Thermometer 66°, barometer 30·1 inches.

Two rabbits, each occupying the space of 48 cubic inches, were inoculated with woorara.

In one of them, when apparently dead, the circulation was kept up by means of artificial respiration. He was placed in the apparatus under the bell-glass, and the lungs were inflated from 50 to 60 times in a minute. At this time the thermometer in the rectum stood at 97°. At the end of 35 minutes, a portion of air was preserved for examination. The thermometer had now fallen to 90°. The heart was still acting regularly.

The second rabbit was allowed to lie dead. When the thermometer in the rectum had fallen to 97°, he was placed under another bell-glass. At the end of 35 minutes, the thermometer had fallen to 90°·5.

The air respired by the first rabbit contained $\frac{1}{16}$ of carbonic acid.

$$\frac{502 + 2 + 52 - 48}{16} = \frac{508}{16} = 31{\cdot}75 \text{ cubic inches of carbonic}$$

acid evolved in 35 minutes, which is at the rate of 54·43 cubic inches in an hour.

Exp. 7. A rab-

Exp. 7. Thermometer 60°, barometer 30·2 inches.

The

bit killed with essential oil of almonds. Revived by artificial respiration, but the cooling was not counteracted, &c.

The experiment was repeated on a rabbit, which had been inoculated with the essential oil of almonds. When he was placed under the bell-glass, the thermometer in the rectum stood at 96°. In a few minutes he gave signs of sensibility, and made efforts to breathe; but as these efforts were at long intervals, the artificial respiration was continued. In half an hour he breathed spontaneously 40 times in a minute. The thermometer in the rectum had fallen to 90°.

The air being examined, was found to contain $\frac{1}{18}$ of carbonic acid.

The rabbit occupied the space of 47 cubic inches.

$$\frac{502+52+2-47}{18} = \frac{509}{18} = 28{\cdot}275$$ cubic inches of carbonic acid evolved in 30 minutes, which is at the rate of 56·55 cubic inches in an hour.

The animal lay as if in a state of profound sleep. At the end of two hours and twenty minutes, from the time of the poison being applied, the thermometer in the rectum had fallen to 79°, and he was again apparently dead; but the heart still continued acting, though feebly, and its action was kept up for 30 minutes longer by means of artificial breathing, when the thermometer had fallen to 76. The carbonic acid evolved during these last 30 minutes, amounted to nearly 13 cubic inches.

Conclusion. That tho' artificial respiration in an animal, of which the brain is without action, will keep up the circulation, and produce the usual changes in the blood, it does not produce heat.

From the precautions with which these experiments were made, I am induced to hope that there can be no material error in their results. They appear to warrant the conclusion, that in an animal in which the brain has ceased to exercise its functions, although respiration continues to be performed, and the circulation of the blood is kept up to the natural standard, although the usual changes in the sensible qualities of the blood take place in the two capillary systems, and the same quantity of carbonic acid is formed as under ordinary circumstances; no heat is generated, and (in consequence of the cold air thrown into the lungs) the animal cools more rapidly than one which is actually dead.

What may be the nature of respiration?

It is a circumstance deserving of notice, that so large a quantity of air should be consumed by the blood passing through the lungs, when the functions of the brain, and those of the organs dependant on it, are suspended. Perhaps it is not unreasonable to suppose, that by pursuing this line of investigation we may be enabled to arrive at some more precise knowledge respecting the nature of respiration, and the purposes which it answers in the animal economy. It would however be foreign to the plan of the present communication to enter into any speculations on this subject, and I shall, therefore, only remark, that the influence of the nervous system does not appear to be necessary to the production of the chemical changes, which the blood undergoes in consequence of exposure to the air in the lungs*.

The

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* The conclusion is directly contrary to that deduced by M. Dupuytren, from a series of curious experiments, made with a view to ascertain the effects which follow the division of the nerves of the par vagum, and it is an object of some importance in the present investigation, to ascertain in what manner the apparently opposite facts, observed by M. Dupuytren and myself, are to be reconciled with each other.

It was observed by this physiologist, that in an animal, in which both the nerves of the par vagum are divided, the blood returned from the lungs has a darker colour than natural, and that the animals, on whom this operation is performed, die sooner or later with symptoms of asphyxia, notwithstanding the air continues to enter the lungs; and hence he concludes, that the changes which are produced on the blood in respiration are not the result of a mere chemical process, but are dependent on the nervous influence, and cease to take place when the communication between the lungs and the brain is destroyed.

M. Provençal, in prosecuting this inquiry, ascertained that the animals subjected to this experiment give out less *carbonic acid* than before.

M. Blainville observed, that the frequency of the inspirations is much diminished; and M. Dumas restored the scarlet colour of the arterial blood by artificially inflating the lungs, and from these and other circumstances, he has arrived at conclusions very different from those of M. Dupuytren.

My own observations exactly correspond with those of MM. Dumas and Blainville. After the nerves of the par vagum are divided, a less quantity of carbonic acid is evolved, the inspirations are much diminished in frequency, and the blood in the arteries of the general system assumes a darker hue; but its natural colour may be restored by artificially inflating the lungs, so as to furnish a greater supply of air to the blood circulating through them.

We may suppose, that, on the division of these nerves, the sensibility

of

The facts now, as well as those formerly adduced, go far towards proving, that the temperature of warm-blooded animals is considerably under the influence of the nervous system; but what is the nature of the connection between them? whether is the brain directly or indirectly necessary to the production of heat? these are questions to which no answers can be given, except such as are purely hypothetical. At present we must be content with the knowledge of the insulated fact: future observations may, perhaps, enable us to refer it to some more general principle.

The temperature of warm-blooded animals is considerably influenced by the nervous system;

We have evidence, that, when the brain ceases to exercise its functions, although those of the heart and lungs continue to be performed, the animal loses the power of generating heat. It would, however, be absurd to argue from this fact, that the chemical changes of the blood in the lungs are in no way necessary to the production of heat, since we know of no instance in which it continues to take place after respiration has ceased.

but the heat is, nevertheless, connected with respiration.

It must be owned, that this part of physiology still presents an ample field for investigation.

Of opinions sanctioned by the names of Black, Laplace, Lavoisier, and Crawford, it is proper to speak with caution and respect, but without trespassing on these feelings, I may be allowed to say, that it does not appear to me that any of the theories hitherto proposed afford a very satisfactory explanation of the source of animal heat.

Where so many and such various chemical processes are going on, as in the living body, are we justified in selecting any one of these for the purpose of explaining the production of heat?

Can any one chemical process be selected as the cause of the heat?

To the original theory of Dr. Black, there is this unanswerable objection, that the temperature of the lungs is not greater than that of the rest of the system. To this objection, the ingenious and beautiful theory of Dr. Crawford is not open; but still it is founded on the same basis with that of Dr.

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of the lungs is either extremely impaired, or altogether destroyed, so that the animal does not feel the same desire to draw in fresh air: in consequence his inspirations become less frequent than natural, and hence arise the phenomena produced by this operation.

Dr. Black, "the conversion of oxygen into carbonic acid in the lungs," and hence it appears to be difficult to reconcile either of them with the results of the experiments which have been related.

Some objections considered.

It may, perhaps, be urged, that as in these experiments the secretions had nearly, if not entirely ceased, it is probable that the other changes, which take place in the capillary vessels had ceased also, and that although the action of the air on the blood might have been the same as under ordinary circumstances, there might not have been the same alteration in the specific heat of this fluid, as it flowed from the arteries into the veins. But, on this supposition, if the theory of Dr. Crawford be admitted as correct, there must have been a gradual, but enormous accumulation of latent heat in the blood, which we cannot suppose to have taken place without its nature having been entirely altered. If the blood undergoes the usual change in the capillary system of the pulmonary, it is probable that it must undergo the usual change in the capillary system of the greater circulation also, since these changes are obviously dependent on and connected with each other. The blood in the aorta and pulmonary veins was not more florid, and that in the vena cava and pulmonary artery was not less dark-coloured than under ordinary circumstances. We may, moreover, remark, that the most copious secretions in the whole body are those of the insensible perspiration from the skin, and of the watery vapour from the mouth and fauces, and the effect of these must be to lower rather than to raise the animal temperature. Under other circumstances, also, the diminution of the secretions is not observed to be attended with a diminution of heat. On the contrary, in the hot fit of a fever, when the scanty dark-coloured urine, dry skin, and parched mouth, indicate that scarcely any secretions are taking place, the temperature of the body is raised above the natural standard, to which it falls when the constitution returns to its natural state, and the secretions are restored.

Dr. Crawford's experiments upon the specific heat of blood *out of the body* and changed may be questioned.

It has been observed, by a distinguished chemist, that "the experiments to determine the specific heat of the blood are of so very delicate a nature, that it is difficult to receive them with perfect confidence"*. "The experiments of Dr. Crawford,

for

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* Thomson's History of the Royal Society, p. 129.

r this purpose, were necessarily made on blood out of the
dy, and at rest. Now, when blood is taken from the vessels,
immediately undergoes a remarkable chemical change, sepa-
ting into a solid and a fluid part. This separation is not
mplete for some time; but whoever takes the pains to make
servations on the subject, can hardly doubt that it begins to
ke place immediately on the blood being drawn. Can expe-
ments on the blood, under these circumstances, lead to any
ry satisfactory conclusions, respecting the specific heat of
ood circulating in the vessels of the body? The diluting
e blood with large quantities of water, as proposed by Dr.
awford, does not altogether remove the objection, for this
ly retards, it does not prevent coagulation, and some time
ust, at any rate, elapse, while the blood is flowing and the
antity is being measured, during which the separation of its
lid and fluid parts will have begun to take place.

General reflections.

More might be said on this subject; but I feel anxious to
oid, as much as possible, controversial discussion. It is my
sh not to advance opinions, but simply to state some facts,
hich I have met with in the course of my physiological in-
stigations. These facts, I am willing to hope, possess some
lue; and they may, perhaps, lead to the developement of other
ts of much greater importance. Physiology is yet in its
fant state. It embraces a great number and variety of phe-
mena, and of these it is very difficult to obtain an accurate
d satisfactory knowledge; but it is not unreasonable to ex-
ct, that by the successive labours of individuals, and the
thful register of their observations, it may at last be enabled
assume the form of a more perfect science.

XII.

bstract of a Memoir upon the Origin and Generation of the Electric Power, whether by means of Friction, or in the Pile of Volta.

By J. P. DESSAIGNES*.

Introduction.

I HAVE the honour to offer to the class, some enquiries into the origin and generation of electricity by friction, as well as in the pile of Volta. This subject must naturally create a lively interest among the learned of Europe, who are occupied on the discovery of the celebrated Volta; and I trust that my zeal for the progress of science, will receive confirmation from my attention to this object. The extent of my researches will not allow of their being read within the time usually allotted for that purpose. I shall, therefore, place my memoir on the table, and confine myself to indicate its principal results.

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* Read before the French Institute, Sept. 23, 1811.

Order of experiments. Electricity by friction—by contact—and by voltaic contact.

My enquiries are divided into four sections; 1. I examine the electricity of idio-electric bodies by friction, in mercury or upon wool; 2. and of the same bodies by simple contact with mercury; 3. and of metals by friction; and 4, that electricity which is produced by the contact of heterogeneous metals, or the pile of volta.

Three applications to mercury: 1. quick immersion; 2. slow immersion; 3. emersion.

Every one knows that idio-electric bodies plunged in mercury, become electric; but it has been hitherto unknown that there are circumstances under which they come out of the mercury without any electric virtue. I shall show those circumstances; but it will be useful in the first place to distinguish three kinds of immersion in mercury. 1. A brisk immersion in the manner of a blow; 2. slow immersion; 3. emersion, which consists in plunging a body in mercury, leaving it there for a longer or shorter time, and afterwards withdrawing it from the fluid. In the two first actions, the entrance and the taking out of the body from the mercury, are successive, and without any interval of repose. I must also observe, that in order to remove all suspicion of humidity, I have always kept those bodies upon which I operated, in a bottle of caustic lime, out of which I took them for each experiment. With these precautions I obtained the following results:

Glass, sulphur, amber, and sealing wax, immersed in mercury, are either electrified or not, according to the temperatures of the bodies and the mercury.

I. In the most favourable times for electricity, glass, sulphur, amber, and sealing wax, at the temperature of 10°. c. are not electric in mercury at 10°. c. by any of the three immersions; neither are they electric in any of the lower temperatures from 10°. c. to —18°. c. provided they be constantly at the same temperature as the mercury. Amber begins to be electric by the blow, and even by immersion, at 10°. c.; sulphur and sealing wax at 15°. c.; and glass at 20. c. but none of them become so by slow immersion at any degree of temperature.

Care

must be taken to leave them in the mercury long enough duce a perfect equilibrium of temperature, and afterwards aw them out very slowly. The same bodies continue to ne electric by the blow, or by quick immersion in eratures superior to the degree at which their power s to be produced. It is remarked, nevertheless, that as bodies and the mercury arrive at much more elevated eratures, their electric power becomes weak, and at last ears, even by brisk immersion, at 80°. c. and 100°. c.

General result.

e may, therefore, establish it as a principle, that these four s are not electrical in mercury, when they are of the temperature with it, and care is at the same time taken no mechanical pressure be exerted, except that of the r weight of the fluid upon the immerged body; and that in the case when the immersion is effected with a strong re, these two united actions do not produce any electrical where the temperatures are equal, and are above 10°. c. nder 80 . c.

Electricity produced by plunging cotton, paper, silk, or wool, in mercury. Here the effects are also governed by the temperature, but differ from the preceding.

The effects are not the same with regard to cotton, paper, and wool, for I have found these very electrical by the immersions, and at all the degrees of heat between 10°. d 80°. c. They continue to be electrical even below 10°. ut by lowering the temperature more and more, cotton is to have its power extinguished in mercury at 3°. c. after ining in it ten minutes; paper is also extinguished at 1°. c. fifteen minutes; silk at 0°. c. after an hour and a half; astly, wool between 5°. and 6°. c. after remaining near hours.

ese four bodies are also extinguished, like the preceding, evated temperatures, and it is even remarked with regard tton and wool, that their power disappears at 60°. c. arises from the humid principle of these bodies, which mes fluid at that degree of heat.

is very observable, that these bodies should also be electric nersion, when they are at the same temperature as the ury at all the degrees of the thermometer, in which their r is not extinguished, whereas, the contrary takes place regard to all the preceding bodies. Their calorific fluid therefore, more natural tension than that of the four first bodies, and does not require an elevation of temperature to urge that of the mercury.

Effects of the third process, called emersion from mercury, upon the first class of bodies.

Singular effect of hot glass plunged in cold mercury.

III. Glass, sulphur, amber, and sealing wax, are constantly electrical, even by immersion, when their temperature is a little higher than that of the mercury; a single degree of difference between the temperature of the rubbing body, and that which is rubbed, is then sufficient to determine an electric state, and this power is more intense, the greater the interval between the two temperatures. Nevertheless, there are limits beyond which it disappears; for example, when a cylinder of glass at 100°. c. is plunged in mercury at 18°. c. the glass then comes forth without electricity, provided the sudden contraction produced by the cold do not crack it, but if it do, the glass becomes extremely electric.

If it crack, it becomes electrified, but otherwise not.

This want of excitability in very hot glass, plunged in very cold mercury, when it does not crack, appears to me to be an effect of the contraction of the glass which prevents the caloric from radiating outward, and forces it to radiate or return into the interior of its substance. It is, no doubt, from this reason, that workmen in glass houses can touch with impunity, a mass of red hot iron in fusion when plunged in water.

Explanation of a point of thermometrical difference.

I have asserted, that a single degree of difference of temperature between the mercury and the rubbed body, is sufficient to determine the electric state; but this is not to be understood but with regard to temperatures remote from the two extremes at which the electric power is extinguished. Thus sealing wax at 8°. c. is weakly electrified in mercury at 0°. c. and strongly at 8°. c. and so likewise at 4° c. it is no longer electrified in mercury at 0°. c. but it continues to be so by the stroke in mercury at 18°. c. The same thing is observed in all the other bodies at some differences in their degrees in relation to their specific heat. Silk, for example, at 0°. c. is still electric in mercury at 15°. c.; it is even so when itself at 4°. c. in mercury at 15°. c. but at 5°. c. it no longer shews any electricity.

Experiments in which the mercury was colder than the immersed body. The effects were much less.

IV. After having determined the influence of heat upon the electric power when it radiates from the body rubbed into mercury, I was desirous of seeing whether the same effect would take place when the heat should pass from the mercury into the immersed body. A tube of glass, particularly when the temperature of the mercury was from 60°. to 80°. acquired no electricity. The same was observed with glass rods; these came out, nevertheless, electric, when the temperature of the mercury was no more than 40°. or 50°. c.; but this electricity is so weak and disproportionate to that which takes place when the body is hot, and the mercury cold, that I have always considered it with surprise. In order to conceive the cause of this difference, it will be sufficient that I should observe, that a stick of glass at 75°. c. requires only two minutes to cool down through 50°. c. in mercury, at 12°. c. whereas, when the same stick at 12°. c. is plunged in mercury at 75°. c. it only causes a loss in the mercury of 4° in the same period.

—— owing to the variation of temperature in the plunged body being greater.

V. To shew the influence of heat upon the electric power still more strikingly, I plunged a thick cylinder of glass in mercury at 80°. c. It first became weakly electric, as I have remarked, and afterwards non-excitable when its temperature was the same as that of the mercury. But sometime afterwards, and when the whole apparatus had advanced in cooling, I found it extremely electrical, and the power afterwards gradually became weaker accordingly, as by the progress of cooling, the internal between the temperatures had become less. When the mercury and the glass were entirely cooled, the electricity was no longer observable. It is evident that the electricity was here produced by the inequality of cooling which took place between the two bodies from their inequality of conducting power for heat.

Similar experiments extended.

VI. I endeavoured to determine the nature of the electricity of all these bodies plunged in mercury. Canton had asserted, that glass comes out of mercury in the positive state. Van Marum and Leroy found it negative. Ingenhousz found it positive by a slow immersion, and negative by a brisk one. After an attentive examination, I found that when the barometer stands high, and the air is inclined to cold, then glass, amber, wax, paper, cotton, silk, and wool, are always negative, whether the immersion be slow or brisk; but that they are, on the contrary, all positive, when the barometer is low, and the air inclined to be warm. It is worthy of remark, that sulphur is constantly positive at the same time when the other bodies were the most strongly negative. I must also observe, that during the whole summer, I found all the bodies positive in impure

Nature or kind of the electricity produced. It depends on the action, and also on the weather.

impure mercury, or such as was alloyed with tin, and negative at the same time in pure mercury.

Other experiments on temperature.

I made several experiments to ascertain the influence of temperature upon positive or negative electricity; one of which I shall mention. On the 10th of July, the wind being N. E. and clear, temperature 21°. c.; at 11h. in the morning, my heated glass rod came out of mercury in the negative state. I raised the temperature of the mercury to 100°. my rod was then without electricity, while the mercury imparted heat to it; but as soon as the whole began to cool, the rod became strongly negative in all its immersed parts. Some time afterwards, and constantly leaving it in the mercury, I found it positive at its extremity, and negative in all the rest. It must be remarked, that the vessel containing the mercury was conical, and the lower part being thin, the cooling was more rapid here than elsewhere. When the mercury was no higher here than 34°. c. the rod came out without electricity, but it continued to be so in the mercury at 26°. c. and constantly negative. Having then heated the rod a little above the mercury, at 34°. I withdrew it positive from the mercury; but it was always negative in mercury at 26°.

It is, therefore, established, that at equality of temperature, the rod is not excitable in mercury; though it is positive when it is only a little warmer than that fluid; and negative, when there is a great interval between the temperatures.

I must remark, that these different degrees of temperature do not change the electric state where the air tends to cold, or the barometer is low; for in the first case the rod is always negative, and in the second always positive.

Friction upon wool.

VII. These influences of the temperature upon the electric power, are not peculiar to mercury; they are observable likewise, in the friction of the same substances upon wool. In fact, if they be cooled in mercury at 12°. their electric power disappears equally in friction. It is not excited in amber until the sixth double friction, in wax at the eighth, in glass at the ninth, and in sulphur at the tenth. In this circumstance, if, after having exacted the electric power of glass, it be left at repose for 30 seconds, it is remarked that it becomes again non-excitable, and it will require four double frictions to reanimate it. It continues afterwards to become electric at each friction, if the action be continued, but is again extinguished if allowed an interval of repose. But at length, after five minutes alternate frictions, and intervals of repose, it becomes permanently electric, however great the interval between the friction. It may be conceived that this effect arises from the glass being a bad conductor, and allowing the heat produced by the friction to pass with difficulty to the centre of its substance.

The excitability of electric power is extinguished by cold, and also by heat.

Not only cold extinguishes the electric power, but an elevated temperature has the same effects. Heat is no less efficacious in changing the nature of electricity; and in order to ascertain this, I varied, or rather repeated, the beautiful experiment of two skeins of silk performed by Beegman, in which we see that each of two skeins perfectly alike, becomes, in its turn, negative, and the other positive, when by the friction to which they are subjected, more heat is given to one than the other, and, consequently, more tension to its electric fluid.

Experiments of producing electricity by mere contact of mercury without immersion.

VIII. Though the effects of heat and cold appear to me to be well shewn by the immersion of idio-electric bodies in mercury; nevertheless, as this kind of electrization gives a mechanical pressure of mercury against the immersed body, I determined to put the influence of temperature in a clearer point of view, by attempting to obtain the electric state by the mere contact of these bodies with the mercury. The following were the results:

Amber, sulphur, and glass.

1. Amber, sulphur, and glass, put into contact with mercury, and without any pressure, do not become electric, while at the same temperature as the fluid. But they become so when heated by the hand, and the slightest difference of temperature is then sufficient for the purpose, particularly when the air is becoming cold, and the barometer stands high.

Cotton, paper, silk, and wool.

2. I have always found cotton, paper, silk, and wool electrical by contact, whatever attention I have bestowed to bring them to an equal temperature with the mercury, provided they be always kept closed in a bottle of caustic lime.

The el. is greater, the greater the interval of temperature.

3. The electricity produced by contact, is always stronger the greater the interval of temperature between the two bodies which touch; yet, if the bodies be heated above 75°.c. and they be applied upon mercury, they acquire no electricity, and do not resume their power until a little cooled.

4. The same is true with regard to the inferior temperature, and those below 0.

A blow will produce electricity when the contacts are at equal temperatures.

5. When bodies have the same temperature as mercury, and the contact produces no electricity, the electric power may be excited by means of a smart blow of the body under experiment upon the surface of the mercury. This blow is sometimes ineffectual when the barometer is low. Here we already see the effect of the barometic pressure upon the electric fluid; but much more evident proofs will be hereafter shewn.

(To be concluded in our next.)

XIII.

Account of the Drainage of a piece of Morass Land, called the Tarn, in the Parish of Clapham, in Yorkshire. By MAJOR B. HESLEDEN. (Soc. Arts XXX.)

Plan of the ground.

THE plan fig. 2. pl. V, describes the direction in which the principal or main drain, as also the cross drains, were severally carried, and A represents a spring of water, B the main drain, CCC, &c. smaller drains, D a small piece of dry ground.

Method of draining.

The land consists of about twenty-one acres, and from its being encompassed on all sides by rising ground, the water was observed to spring from the bottom of the hill; consequently, the first drain was taken along its base and boundary of the Tarn, so as to receive the water on its first approach; the others were taken in the same direction, some of which, near the outward side, were made at the distance from seven to ten yards; but near the centre, at a greater distance, (viz.) ten to fifteen yards from each other, according to the dryness of the land. The principal, as well as the cross drains, were finished in the best possible manner, the bottom of them being invariably flagged, or laid with flat stones, (except in a few instances that happened to be firm clay), previous to its being walled on both sides, or soughed, then covered with flat stone, and afterwards filled to the top with earth and sod, which would be above the stone from one foot to half a yard in thickness.

This mode of draining, the Major would recommend to be always adopted, provided a sufficient quantity of stone can be procured, even if the expense attending it should be somewhat great, since it evidently must ensure its durability, almost for ever,—when in the ordinary way of draining, without first flagging, or laying the bottom with stone, it will in no great space of time give way by undermining the walling, or soughing part, besides being more liable to fill or choke up with earth or sediment. It would also be advisable to adopt (as was done in this instance with good effect) the letting in lots, of a given number of roods each, the cutting, stone-digging, soughing, or walling, &c. to different workmen; as best calculated to ensure the well finishing and due performance of each work.

Expences.

The draining being completed, covered, sodded, and levelled, and the same covered with about three thousand horse loads of lime, and after adopting the greatest economy in the expenditure of this undertaking, the whole proved nearly as follows:

	L.	*s.*	*d.*
The main drain, cutting and blowing up of the rock, and carrying through the hill of the extent and depth so as to gain a sufficient fall......	92	15	0
The cross and other drains in the Tarn..........	181	7	0
Covering the whole with lime.................	91	0	0
	*L*365	2	0

Value of the land recovered gives 18 or 20 per cent. profit.

The value of the land in its improved state, in grass, was calculated to be worth, for the first two or three years, from two pounds ten, to five pounds fifteen shillings per acre; but the proprietor has not the least doubt, from his experience in laying lime upon the surface of land of that description, and in the same neighbourhood, that from the end of the first three years, it will be worth afterwards, and for some time, (for grazing or fattening of cattle) from three pounds ten, to three pounds fifteen shillings per acre, and, consequently, will pay for the money so expended, at least eighteen or twenty per cent.

XIV.

Respecting the Action of coloured Rays upon a Mixture of oximuriatic Gas, and hidrogen Gas. By *Mr.* SEEBECK*.

Oximur. gas and hidrogen explode by solar light;

A MIXTURE of oximuriatic gas and hydrogen gas being exposed to the solar light, was suddenly decomposed by Messrs. Gay Lussac and Thenard. (See Recherches Physico-Chimiques, tom. 2, p. 189.) I repeated this experiment with success by means of the gasses which I had collected over hot water. I afterwards introduced these gasses under a glass vessel of a reddish yellow colour, and in another of a deep blue, which I exposed to the solar rays. Under the blue glass the decomposition immediately took place, without, however, any appearance of explosion, and in a minute at most it was terminated, and the glass was filled with water for the most part.

but not under coloured glass: blue light decomposes them much sooner than red, and neither with explosion.

Under the red glass, on the contrary, the decomposition took place very slowly. After twenty minutes of exposure to strong solar light, very little water had risen in the glass. This mixture of gas from the red glass was then introduced into the white glass, and exposed to the rays of the sun. No explosion took place; but in a few minutes the glass became filled with water. These experiments were frequently repeated, and always with the same result.

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* Translated from Schweigger's Journal of Chemistry, II, 263, by Vogel; from whose article in the Annales de Chimie, LXXXII, 328, I have extracted it.—N.

SCIENTIFIC NEWS.

Geological Society.

AT a meeting of this Society, on January 1, 1813, (the president in the chair) the reading of Mr. Philips's paper "on the Veins of Cornwall" was concluded.

The metalliferous veins of the Herland and Drannack mines run E b N and W b S, and the cross courses run N b W and S b E. The rock or country which they traverse is Schist, in some places so hard as to require being blasted. The width of most of the metalliferous veins varies from two inches to six inches: whenever exceeding this latter measure, they have been found soon after to divide and pass away in mere strings. A contre or oblique vein traverses these mines in a direction W b N and E b S, varying in width from one to three feet. Near the surface it was found to abound in blende and iron pyrites, but lower down afforded large quantities of copper ore. Whenever it intersected the metalliferous veins, the place of junction formed one lode for about eight fathoms in length, and three or four in width. The contre was heaved by the cross courses, and these latter, at the place of intersection, are found to be not only enlarged but impregnated with ore. The contents of the cross courses are clay, quartz, or a mixture of both. It was in one of these cross courses, at the place of its junction with one of these metalliferous veins, that the celebrated deposit of silver was found mingled with galena, with iron pyrites, with bismuth, cobalt, and wolfram; and these substances were also found in those parts of the vein adjacent to the cross course.

Huel Alfred is in immediate contact with the mines just mentioned, and is at present one of the richest and most profitable copper mines that Cornwall can boast of. The great deposit of ore is contained in a contre from nine to twenty-four feet wide, which is considered as the continuation of that in Herland mine. The contre traverses a regular east and west vein, and it is remarkable that the ore, abundant as it is, has hitherto been found only in one mass at the depth of 117 fathoms at the point of junction of the contre and of the vein, giving off a branch 110 fathoms in length, along the eastern part of the same vein.

Another singular circumstance in this mine is, that one of the cross courses is heaved and intersected by an E and W vein.

Since the beginning of 1801, there have been sold about 45,000 tons of copper ore, the produce of Huel Alfred, for the sum of about 350,000l. of which the profit, divided among the adventurers, has amounted to about 120,000l.

January 15th.

The president in the chair.

A paper by William Conybeare, Esq. M. G. S. "On the origin of a remarkable class of organic impressions occurring in nodules of flint," was read.

This paper, which is chiefly occupied by detailed explanations of the drawings by which it is accompanied, relates to a class of substances thus characterized by Mr. Parkinson, in the second volume of his work on organic remains.

"Small round compressed bodies not exceeding the eighth "of an inch in their longest diameter, and horizontally disposed "are connected by processes nearly of the fineness of a hair, "which pass from different parts of each of these bodies, and "are attached to the surrounding ones; the whole of these "bodies being thus held in connexion." p. 75.

Mr. Parkinson conjectures, that the formation of these bodies has been the work of some polype similar to those by which the common zoophytes have been constructed, and, therefore, classes them among fossil corals of unknown genera. He observes, however, at the same time, that his reason for this arrangement is only a very slight analogy, as the objects in question differ materially from every known zoophyte, recent or fossil.

Mr. Conybeare having been so fortunate as to obtain several specimens of this fossil in a much better state of preservation than usual, shews clearly that they occur between the bony plates of a large bivalve shell, the Ostreo-pinnite of Walch, and in a similar situation in fragments of a striated shell, one of the patellites of Da Costa, which more probably, however, belongs to the genus ostrea. Similar substances have also been observed on the surface of a cast of the echinus. The matter of which these bodies are composed is flint, and they are supposed by Mr. Conybeare to be casts of the cells of some minute parasitical insect inhabiting the substance of the shells of certain species of the testaceous molluscæ, and probably deriving hence its nutriment either in whole or in part.

The anniversary meeting of the Society for the Election of Officers, &c. was held on Friday, the 5th of February, when the following members were elected.

President

The Hon. Henry Grey Bennet, M. P. F. R. S.

Vice-Presidents.

Sir Abraham Hume, Bart. M. P. F. R. and L. S.
Robert Ferguson, Esq. F. R. S.
Sir Henry Englefield, Bart. F. R. and L. S.
John Mac Culloch, M. D. F. L. S.

Treasurers.

William Haseldine Pepys, Esq. F. R. S.
Samuel Woods, Esq.

Secretaries.

Leonard Horner, Esq.
Arthur Aikin Esq.

Foreign Secretary.

Samuel Solly, Esq. F. R. S.

Council.

The Council consists of the above officers of the society, and of twelve other ordinary members.

Alexander Apsley, Esq.
William Blake, Esq. F. R. S.
J. G. Children, Esq. F. R. and L. S.
Samuel Davis, Esq. F. R. S.
James Franck, M. D.
G. B. Greenough, Esq. F. R. and L. S.
Alexander Jaffray, Esq.
James Laird, M. D.

James Parkinson, Esq.
Smithson Tennant, Esq. F. R. S.
Henry Warburton, Esq. F. R. S.
William Hyde Wollaston, M. D. Sec. R. S.

Keeper of the Museum and Draughtsman.
Mr. Thomas Webster

February 19*th.*

The president in the chair.

John Bostock, M. D. and Thomas Stewart Trail, M. D. of Liverpool, were elected members of the society.

A paper by John Taylor, Esq. M. G S on the economy of the mines of Cornwall and Devon, was read.

The subjects treated on in this paper are,

1. The nature of the agreements between the owner of the soil and the mine-adventurers.

2. The arrangements between the partners, or the mine-adventurers themselves, and the system of controul and management appointed by them.

3. The mode of employing and paying the miners and workmen in use among the agents of the principal concerns.

4. The purchase of materials for carrying on the undertaking.

5. The sale of the ores from the mine-adventurers to the smelting companies.

1. The regulations of the *stannary* laws refer only to mines of tin; hence the search after, and working lodes of copper, lead, and other metals, is left open to such conditions as the adventurers and the lord of the soil can mutually agree upon. In general, the lord grants a lease for twenty-one years, determinable; however, at any time on his part if the mine should not be effectually worked. In return he requires a certain proportion, varying according to circumstances from an eighth to a thirty-second part of the ore, to be delivered to him in a merchantable state, or its value in money. He stipulates for a power of inspecting the works at all times, and binds the adventurers to maintain and leave, at any determination of the grant, all the shafts, adits, and levels, perfect and in good condition as to timbering.

2. The adventurers divide the whole concern into sixty-four

shares, which they distribute among themselves, and those who are allowed to join them, in various proportions. At the end of every two or three months, a general meeting of the adventurers is summoned, a statement of the accounts is laid before them, and the profit or loss is distributed to each, according to the amount of his shares. The general detail of management is usually delegated to one person, under whom are subordinate managers, called captains, selected among the working miners for their skill and character.

3. The work of the mines, both on the surface and below ground, is almost universally contracted for by the piece, at a kind of public auction held at the end of every two months; an accurate survey and measurement of the whole being previously taken by the captains. The lowest bidder has the *set*, and, in order to execute it, he associates to himself from one to eleven men, women, or children, according to the nature of the work. An account is then opened between the principal captain and the contractor, in which this latter is credited with all the tools, candles, gunpowder, and subsistence-money required by himself and his gang during the term; at the end of which the tools, and articles not used, are returned; the account is balanced, and the gain or loss on the contract is declared to the persons interested.

4. If materials for the use of the mine are purchased from those holders of shares who deal in the articles wanted (as is not unusual) great vigilance is required in the other proprietors to check the natural temptations to charge exorbitant prices, or to encourage a wasteful consumption.

5. The smelting companies for copper have seldom any share in the mines. There are about fifteen copper companies, all of which have agents and assay offices in Cornwall, though the smelting itself is carried on at Swansey. A weekly meeting is advertized to be held at some place near the principal mines, where the ores on hand, allotted into suitable parcels, (the produce of one mine being kept separate from that of another) are offered for sale. Previous to the day of sale, the persons intending to purchase attend at the mines for the purpose of taking samples, which are immediately put into the hands of the assay-masters. The agents for the smelting companies being thus furnished with the requisite information,

attend at the meeting, and each hands up to the chairman a note or ticket, containing the price per ton which he is disposed to give; the chairman then reads aloud the various offers, and the highest bidder is declared the purchaser.

Models of all the most Interesting Parts of the High Peak of Derbyshire.

MR. ELIAS HALL, *Fossilist and Petrefaction-Worker*, of Castleton, near Tideswell, in Derbyshire, has announced, that since the *mineral survey* of the county of Derby was undertaken for the board of agriculture, by *Mr. John Farey*, sen. and particularly since the publication of the first volume of his "Report on Derbyshire" by the Board; Mr. H. has assiduously applied himself to an examination of all the mineral limestone district and its vicinity, to the northward of Winster and Hartington, and to the carving out and completing of a *model* of its curious and rugged surface, under the patronage of his Grace the present *Duke of Devonshire*, and the Right Hon. *Sir Joseph Banks*, Bart.

On exact casts from this model he has contrived (as on Mr. Farey's mineral Maps) to represent, by colours, the eleven lowest of his principal rocks and strata of this district; viz. 1, fourth limestone (*ochre yellow*);—2, third toadstone (*red*);—3, third limestone (*grey blue*);—4, second toadstone (*bright yellow*);—5, second limestone (*green*);—6, first toadstone (*very dark blue*);—7, first limestone (*grey white*);—8, limestone shale (*reddish brown*);—9, first or millstone grit (*yellow*);—10, first coal shale (*dark brown*);—and 11, second grit rock (*dark brown*).—These several colours being painted on a fillet, in their proper order, by the side of actual *specimens* taken from the eleven rocks and strata above mentioned, are arranged and fixed on the E. side of the model, interspersed with other specimens of the chert, black marble, shale-freestone, coals, entrochi, &c. which belong to such strata.

Besides which, the ranges of all the principal *mineral veins* are represented, on the model, by *blue* lines for *rake veins*; and broad *blue* lines with *yellow* dots on them, for *pipe veins*; and specimens of lead ore in each limestone rock are given.

The great limestone and the Bakewell *faults* (of Mr. Farey's Report, Vol. I. pp. 280 and 290,) are represented by a broad *red* line with *black* dots on it.

The turnpike roads are represented by small elevated lines; and the towns and villages, by small round elevations.

Printed labels are affixed to the several towns, villages, roads, hills, valleys, strata, mineral veins, caverns, &c.

The superficial scale of this model is one inch and a quarter to a mile. The scale for heights and depths necessarily exceeds the other, in order to give every hill and valley as nearly as possible the appearance that it had on the spots, where the carving was the greater part of it executed.

The colours are painted in oil, so as to be permanent, and admit of the model being cleaned from dust, &c.; and the whole is inclosed in a strong deal box, 20 inches long (from N to S,) 19 inches wide (from E to W,) and three inches deep, with a lid which takes off when the model is in use, and on the under side of which this description may be pasted and preserved.

For more readily understanding the internal parts of the district represented in the model; it should be observed, that the limestone-stone shale (reddish *brown*) occupies all the borders of the model, (but sometimes with first grit and coal shale upon it,) except for about six inches near its bottom, or S. end, between Sheen-Hill and Hartle-Moor: that this scale has an easy dip, or declines gently on the W. N. and E. sides, in those several directions, or with an easy rise towards the limestone and toadstone districts, whose strata have a general and rather a rapid dip towards the E. The four limestone rocks, coloured grey *white*, *green*, grey *blue*, and ochre *yellow*, dip successively under the eastern shale, and each other, in this order: and the three toadstones, coloured dark *blue*, bright *yellow*, and *red*, dip also to the E. between the limestone rocks.

The coloured patches and rings will point out the several *hummocks* and *denudated patches* of strata, that are detached from the masses or surfaces of the seven strata of limestone and toadstone which are mentioned above.

Mr. Hall's own examination of the *strata* of the considerable district comprised in his model was separately conducted, and afterwards compared with Mr. Farey's report and manuscript map; and every spot, wherein any difference of the two surveys appeared, has been visited again and again; all the old and best-informed miners have been consulted, and by repeated correspondence, and the liberal and ready communications of Mr. Farey, he considers himself as warranted to present his models, as faithful representations of the numerous and highly curious phenomena which the High Peak presents; any of which he is ready to explain minutely on the spots, and, in other respects to assist the investigations of curious travellers who may be anxious to examine and verify these facts, and wish to engage his personal assistance for such purpose. It is also a part of Mr. Hall's professional business to make and label ample *specimens* of all the various *mineral productions of the Peak Hundreds*; carefully noting their precise localities, and their places in the strata or veins (a species of information too rarely met with, even in the best mineral collections.) He always keeps a large collection of the *Derbyshire minerals*, for sale, collected almost entirely by himself.

Mr. Farey, from a desire to promote mineral science, and to serve Mr. Hall, has consented to keep some of his models, at his house, No. 12, *Upper Crown-street, Westminster, London*, for inspection, and sale, at eight guineas each. They may also be had, on these terms, of Mr. Hall himself, as above, or by application to him by letter.

Mr. Bakewell will commence a course of geological lectures in March, at Willis's rooms, King-street, St. James's, designed to illustrate the geology and mineralogy of England, and particularly intended to direct the attention of landed proprietors to the neglected mineral treasures on their own estates. Mr. Bakewell also intends shortly to publish, in 1 Vol. 8vo. a work entitled Outlines of Geology, with observations on the Geology of England.

Speaking

Speaking Automaton or Machine.

Mr. **Robertson**, whose name has frequently appeared in the Annales de Chimie, but is better known to the public as an aeronaut in Denmark, has, it is said, contrived a speaking figure, which he exhibited a few months ago at Paris. It articulates the words *Papa, Mamma,* and *Vive Napoleon,* and daily improves in power, from (as may be supposed) the practice of the person who works it.

On this occasion I would remind some of my readers, who may have remembered the automaton chess player, which was exhibited in St. James's Street, about thirty years ago by the Baron Kempellen,—that that mechanic shewed, in a kind of half private exhibition in his parlour, after the games at chess were over; an instrument which spoke. I was present at one of these performances The Baron said the machine was not then completed. It was a kind of box which he brought out and placed upon a table. Speaking without any memorandums at so considerable a distance of time, I judge its dimensions to have been about two feet in length, one foot wide, and eight or nine inches deep. It had no lid; but we were prevented from seeing the inside by a cloth which covered it. The Baron put his hands into the box under the cloth, so that his right arm was disposed longitudinally in the box, and seemed to press a pair of bellows: the other hand was put in, crosswise at the end, near the place of the right-hand, and seemed to be employed with keys or some apparatus, or perhaps both hands may have been so employed. When he caused the instrument to speak, he raised his right elbow and gradually pressing it down, the sound was heard. It was a clear monotonous sound as if from a single pipe about the pitch of D, above the middle C, concert pitch; and the words *papa* and *mamma* were uttered, very distinctly in a slow drawling manner; that is to say there was a want of the usual modulation of speaking tones, and the sound fell off in its intensity towards the end. After several other words had been spoken, a lady asked in French if it could not speak sentences, and the Baron asked what it should say: she answered *que se suis mechante*, and the instrument said *vous etes mechante, mais vous etes aussi bonne.*

Kratzenstein has given some account of the principles of an engine of this kind in a memoir extracted in the Journal de Physique, and Dr. Young has cursorily mentioned the subject in his lectures, with some diagrams.

Quantity of Spirits and other fluids ascertained by Weight.

MRS. LOVI has lately communicated a memoir to the Edinburgh Institute, upon the advantages in point of accuracy of measuring fluids by weight instead of using vessels of known magnitude. She is mentioned as patentee of the areometrical beads, which have been now upon sale in London for a considerable number of years, and consist of small glass balls hermetically sealed, having their specific gravities written upon them. The principal objection to these is their number and brittleness, and, perhaps, the difficulty of making them of so small a size, with as much accuracy as the larger ball, of the usual floating instrument.

With regard to the practice recommended of weighing fluids instead of measuring them, it is grounded on the considerations, 1. That weights are usually made with more exactness than measures. 2 The measuring multiplies error by a series of operations, 3. and it has been questioned whether a single measure of a fluid could be had to the same degree of precision as a mass determined by weight; though by the common figure given to the copper measures which terminate in a neck or throat having a small surface, the precision required in business may, no doubt, be had.—Considerations of temperature affect both methods alike, and I apprehend that, though the temperature of spirits is attended to, in determining their strength, yet it is neglected in taking the measures of quantity; which it is liable to affect as far as 2 per cent, or one gallon in 50, and ought, therefore, to be considered.

I have seen oil sold retail by the gallon weight in London, which is certainly very fair for the buyer, who might else in that adhesive fluid, lose as much as hangs to the vessel every time it is emptied.

New Publications.

The Gentleman's Mathematical Companion, for the Year 1813; containing answers to the last year's enigmas, &c. &c. Price 2s. 6d.

Elements of Universal Geography, ancient and modern; with historical, classical, and mythological notes. By A. Picquet. 12mo. 5s.

A Sketch of the Sikhs, a singular nation, who inhabit the provinces of the Penjah, situated between the rivers Jumna and Indus; by Sir W. Malcolm, 8vo. 8s. 6d.

Asiatic Researches; or, Transactions of the Society instituted in Bengal. Vol. XI. octavo 18s. or quarto two guineas.

Topographical Dictionary of Yorkshire. By W. Langdale. 8vo. 10s. 6d.

The Picture of London for 1813. 6s. 6d.

Travels in South America. 4to. 2l. 2s. forming vol. XIV of Pinkerton's General Collection of Voyages and Travels.

General Collection of Voyages and Travels. Part LVIII. 4to. 10s. 6d.

Journal of a Residence in India; by Major Graham. 4to. 1l. 11s. 6d. boards.

Rees's New Cyclopedia. Vol. XXIII. Part I. 1l. or large paper, 1l. 16s.

Correct Tide Tables for the Year 1813, shewing the true time of high water in the morning and afternoon of every day in the year, for all the principal ports and places in Europe and America. By William Adams. 1s.

* * *

Sir H. Davy will soon publish Elements of Agricultural Chemistry, in a Course of Lectures delivered before the Board of Agriculture.

A new edition of Smeaton's Account of the Building of Edystone Light-house has been announced, and is considerably advanced.

Professor Playfair is printing the second part of his Outlines of Natural Philosophy; and also a new edition of his Illustrations of the Huttonian Theory.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1813.

ARTICLE I.

Experiments on the comparative Strength of Men and Horses, applicable to the Movement of Machines. By M. SCHULZE*.

Importance of measuring the force of men and animals as first movers

THOSE who have had occasion to construct machines intended to be moved by men or animals, are sufficiently aware how important it is to be acquainted with the quantity of power that can be attributed to either of them, in order to estimate with accuracy the effect which it is proposed to obtain from the machine. It is well known, that the arrangement of the whole depends entirely on the ratio of the velocity of the motive force to the resistance. This was the reason that long ago induced experimentalists to take the trouble of determining the strength as well as the velocity exerted by men and animals, when they are made to move machinery; and the results they obtained, which have been commonly made use of in computing the effect of machines, are, that men exert from twenty-seven to thirty pounds, with a velocity of from one and a half to two feet per second; and that a horse has about seven times more strength than a man, with a velocity of from four to six feet per second.

Formulas of

These are the data which we have been obliged to use when-

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* Memoirs of the Royal Academy of Sciences of Berlin for 1783.

Euler for determining the effects of machines moved with different velocities, &c.

ever it became necessary to compute the effect of a machine moved by men or horses. It is evident that the force must be diminished when the velocity is increased, and *vice versâ*: but we are not yet certain of the method of finding the ratio of the diminution or augmentation of this force to the velocity. Euler has given us two different formulæ to compute this ratio: but no one has hitherto attempted to verify by experiment which of them is to be preferred, although they differ very considerably from each other. If we put P for the absolute force which takes place when we simply consider equilibrium, C the absolute velocity which takes place when the man or animal moves freely, and without being overcome by the resistance, p the relative force, and c the corresponding velocity, we have by the first of these formulæ,

$$p = P\left(1 - \frac{c}{C}\right)^2 \text{; whereas the second gives us } p = P\left(1 - \frac{c^2}{C^2}\right)$$

As I am obliged now more than ever to attend to a number of machines, and to compute their effect, it therefore concerns me very much to know exactly in what manner to estimate, compare, and fix the strength and velocity of men and animals, which are used for moving various machines, proper for different purposes.

With this view I made, with considerable care, the experiments I am now about to detail, which of course would have been very expensive, had I not had some facilities which other persons may not possess.

Experiments with men.

To make the experiments on human strength, I took promiscuously twenty men of different sizes and constitutions, whom I measured and weighed; the result of which is given in the following table:

Their sizes and weights.

Order.	Size.			Weight.	Order.	Size.			Weight.
1	5'	3"	4"'	122	11	5'	9"	7"'	132
2	5	2	3	134	12	5	1	4	157
3	5	7	2	165	13	5	3	2	175
4	5	5	0	131	14	5	4	1	117
5	5	11	2	177	15	5	10	8	192
6	6	0	4	158	16	5	0	3	133
7	5	8	3	180	17	4	11	2	147
8	5	2	1	117	18	5	3	9	124
9	5	4	8	140	19	5	6	0	163
10	5	0	4	126	20	5	10	1	181

To find the strength that each of these men might exert to raise a weight vertically, I made the following experiments:

Experiments upon their strengths

I took various weights, increasing by 10lbs. from 150lbs. up to 250lbs. All these weights were of lead, having circular and equal bases. To use them with success in the proposed experiments, I had at the same time a kind of bench made, in the middle of which was a hole of the same size as the base of my weights: this hole was shut by a circular cover, which effected this purpose when pressed against the bench, but at other times was kept at about the distance of a foot and a half above the bench, by means of a spring and some iron bars. To prevent the weight with which this cover was loaded during the experiment, from forcing down the cover lower than the level of the surface of the bench, I had several grooves made in the four iron bars, which sustained the cover at any height at which it might arrive by the pressure of the springs, as soon as the pressure of the weight ceased.

in raising a weight vertically.

After having laid the 150lbs. on the cover, and the other weights in succession, increasing by 10lbs. up to 250lbs. I made the following experiments with the men whose size and weight are given above, by making them lift up the weights as vertically as possible all at once, and by observing the height to which they were able to lift them. The following table gives the heights observed for the different weights marked at the head of the table.

	150	160	170	180	190	200	210	220	230	240	250
1	7″ 9‴	6″ 4‴	4″ 11‴	4″ 4‴	3″ 8‴	2″ 8‴	1′ 1‴				
2	7 10	6 6	5 7	4 7	3 11	2 5	0 5				
3	7 9	7 8	6 5	5 9	4 11	4 0	3 0	1″ 7‴	0″ 3‴		
4	8 3	7 6	7 2	5 10	5 3	4 7	4 0	3 8	2 1	1″ 4‴	
5	12 4	11 1	9 7	8 5	7 10	7 1	5 10	4 7	3 2	1 3	
6	14 5	14 0	13 5	12 8	11 5	10 1	8 6	6 6	4 1	0 1	
7	12 11	11 3	10 5	9 3	8 1	6 9	5 3	3 8	1 11	0 2	
8	11 9	10 2	9 4	8 11	8 1	6 11	5 10	5 1	3 2	1 0	
9	9 5	8 3	7 1	5 6	4 1	2 9	1 3				
10	8 1	6 5	4 7	3 9	2 5	1 7	0 4				

Results.

This table proves to us, that the size of the men employed to raise the weights vertically, has considerable influence on the height to which they severally brought the same weight. We find also by this, that the height diminishes in a much more considerable ratio than the weight increases; and we may therefore conclude, that it is advantageous to employ large men when it becomes necessary to draw vertically from below upwards; and, on the contrary, it is more advantageous to employ men of considerable weight, when it is required to lift up loads by means of a pulley, about which a cord passes, which the workmen draw in a vertical direction, from above downwards. To find the absolute strength of these men in a horizontal direction, I took the following method:

Having fixed over an open pit a brass pulley, extremely well made, of fifteen inches diameter, whose axis, made of well-polished steel, to diminish the friction, was three-fourths of an inch in diameter; I passed over this pulley a silk cord worked with care, to give it both the necessary strength and flexibility. One of the ends of this cord carried a hook to hang a weight to it, which hung vertically in the pit, whilst the other end was held by one of the twenty men, who, in the first order of the following experiments, made it pass above his shoulders; instead of which, in the second, he simply held it by his hands.

I had taken the precaution to construct this in such a manner, that the pulley might be raised or lowered at pleasure, in order to keep the end of the cord held by the man always in a horizontal direction, according as the man was tall or short, and exerted his strength in any given direction.

I had made the necessary arrangements, so as to be able to load successively the basin of a balance which I had attached to the hook at the end of the cord which descended into the pit, whilst the man who held the other end of the cord employed all his strength without advancing or retracting a single inch.

Experiments with men pulling horizontally.

The following table gives the weights placed in the basin when the workmen were obliged to give up, having no longer sufficient strength to sustain the pressure occasioned by the weight. To proceed with certainty, I increased the weight each time by five pounds, beginning from 60, and intervals of time, having always precisely a space of ten seconds between them. The result of these observations, repeated several days in succession, is contained in the following table:

When the cord passed over the shoulders of the workmen:

Order.	lbs.	Order.	lbs.	Order.	lbs.	Order.	lbs.
1	95	6	100	11	95	16	95
2	105	7	115	12	100	17	100
3	110	8	105	13	110	18	90
4	100	9	95	14	90	19	110
5	105	10	90	15	110	20	105

When the cord was simply held before the man:

Order.	lbs.	Order.	lbs.	Order.	lbs.	Order.	lbs.
1	90	6	100	11	90	16	90
2	105	7	110	12	90	17	90
3	105	8	100	13	100	18	85
4	90	9	90	14	85	19	100
5	95	10	85	15	105	20	100

Results. These two tables show, that men have less power in drawing a cord before them than when they make it pass over their shoulders: it shows us also that the largest men have not also the greatest strength to hold, or to draw in a horizontal direction by means of a cord. To obtain the absolute velocity of these twenty men, I proceeded as follows:

Marching of men for considerable times. Having measured very exactly a distance of 12,000 Rhinland feet, in a plain nearly level, I caused these twenty men to march with a good pace, but without running, and so as to continue during the space of four or five hours. The following is the time employed in describing this space, with the velocity resulting from each of them.

Ord.	Time.	Veloc.	Ord.	Time.	Veloc.	Ord.	Time.	Veloc.
1	40′ 18	4′ 94	8	40′ 9	4′ 99	15	36′ 17	5′ 51
2	41 12	4 85	9	40 20	4 96	16	41 28	4 82
3	39 8	5 53	10	40 51	4 90	17	42 25	4 71
4	39 40	5 04	11	36 17	5 51	18	40 19	4 98
5	34 19	5 83	12	38 11	5 24	19	39 57	5 01
6	35 11	5 68	13	38 5	5 25	20	37 51	5 29
7	38 7	5 25	14	37 1	5 40			

It is necessary to mention, with regard to these experiments, that I took care to place, at certain distances, persons in whom I could place confidence, in order to observe whether these men marched uniformly and sufficiently quick without running.

Having thus obtained, not only the absolute force, but the absolute velocity also, of several men, I took the following method to determine their relative force.

The same with an additional resistance against the men.

I had made use of a machine composed of two large cylinders of very hard marble, which turned round a vertical cylinder of wood, and moved by a horse, which described in its march a circle of ten Rhinland feet. This machine appeared to me the most proper to make the following experiments, which serve to determine the relative strength that the men had employed to move this machine, and which I use hereafter to determine which of Euler's two formulæ ought to be preferred.

To obtain this relative force, I took here the same pulley which served me in the preceding experiments, by applying a cord to the vertical cylinder of wood, and attaching to the other end of this cord, which entered into an open pit, a sufficient weight to give successively to the machine different velocities.

Having applied in this manner a weight of 215lbs. the machine acquired a motion which, after being reduced to an uniform motion, taking into account the acceleration of the weight of the friction, and of the stiffness of the cord, gave 2·41 feet velocity; and having applied in the same manner a weight of 220lbs. the resulting uniform motion gave a velocity of 2·47 feet. I only mention these two limits, because they serve as a comparison with what immediately follows. I began these experiments with a weight of 100lbs. and increased it by five every time, from that number up to 400lbs.

I made this machine move by the seven first of my workmen, placing them in such a way, that their direction remained almost always perpendicular to the arm on which was attached the cord which passed over their shoulders in an almost horizontal direction.

Results.

Thus situated, they made 281 turns with this machine in two hours, which gave for their relative velocity $c = 2·45$ feet per second. We have also the absolute force, or P, from these seven men by the above table = 730lbs. and their absolute velocity, or $C = 5·30$ feet.

Therefore, by substituting these values in the first formula, we find the relative force $p = 205$lbs. which agrees very well with what we have just found above.

If instead of this first formula, the second be taken, it gives $p = 153$lbs. which is far too little.

Euler's first formula preferred.

By this it is evident, that the first of Euler's two formulæ is to be preferred in all respects. I have also made a great number of combinations, and I almost always found the same effect.

Dividing the 205lbs. which we have just found, by seven, the number of workmen, we get 29lbs. for the relative force, with 2·45 feet relative velocity for each man, which is rather more than the values commonly adopted in the computation of machinery. A number of other observations on different machines, which I intend to relate another time, have given me the same result; that is to say, we must value the mean human strength at 29 or 30lbs. with a velocity of 2½ feet per second.

A man's strength 29 or 30, with 2½ ft. veloc. per second.

To obtain the ratio of the strength of a horse to that of a man, I had the same machine moved by a horse, without altering any thing; and I found by ten different horses which I used successively, that a horse makes 603 turns in two hours instead of 281; therefore, by supposing the static motion of a horse seven times greater than that of a man, we find that the former has 5·3 feet per second of velocity.

Horses exceed men 14 times in drawing.

By this it is evident, that the effect of a horse is fourteen times greater than that of a man, or, which amounts to the same thing, fourteen men must be used instead of one horse. Hence it appears, that it is much more advantageous to employ horses than men in moving machines, if other reasons did not require us to prefer men.

I have also made a number of other interesting observations on horses and oxen, which are likewise used in moving machines; but as I am now waiting for observations of this kind, which other persons are making according to my plan, I shall reserve them for another memoir.

II.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature:

By Professor J. BERZELIUS.

(Continued from p. 166.)

Indication of experiments made upon the combinations of metals with oxigen and with sulphur for determining their composition, their electro-chemical nature, and relations.

AFTER this general review of the changes which appear to be necessary in the theory of chemistry, I shall have the honour to present to the academy the results of some experiments upon the combinations of various metals with oxigen and with sulphur, made with the intention partly of determining their composition with greater precision, in order to refute certain incorrect notions respecting their nature, and partly to ascertain the electro-chemical nature of those metals, as well as the place they ought to occupy in the system among the other combustibles. Much remains yet to be done on this subject, because the field to be explored is so extensive, that each individual step appears relatively of small magnitude. My researches have been made upon the oxides of tin, tellurium, gold, platina, palladium, lead, zinc, and manganese; and at my request the following metallic oxides have been analysed, namely, those of cerium by M. de Hisinger; those of nickel and cobalt by M. Rathoff; that of bismuth by M. Lagerhjelm; and those of mercury by M. Seffiroud; and these chemists solicit the honour to publish their works in the Memoirs of the Academy.

Ammonium?

I should also have wished to add to these experiments that of the production of an amalgam of ammonium produced an anhydrous ammoniacal salt; and though in my experiments on that subject, an amalgam of kalium has produced an amalgam of ammonium in the subcarbonate of ammonium, prepared with the carbonic acid gas and dried ammoniacal gas, I shall not venture to present the same to the Acadamy as a well-determined result, because I have not yet had an opportunity of examining to what degree I may have succeeded in operating with materials perfectly deprived of water. It is, nevertheless, clear, that the success of an experiment of this nature would be decisive as to the nature of ammonium.

I. Concerning the Oxides of Antimonium.

On the number, &c. of antimonial oxides.

Notwithstanding the labours of chemists have, perhaps, been more frequently employed upon this metal than upon any other, we have hitherto possessed but few data respecting the number and the nature of its oxides; and the information given in elementary works is often contradictory the one to the other. The chemists who have operated the most successfully upon these oxides are, MM. Proust, Thenard, and Bucholz. Thenard, guided by the principle of unlimited combinations advanced by the illustrious Berthollet, found that antimony produced six different oxides, that is to say, one black, one chestnut brown, one greyish white and fusible, one white and not fusible, one orange, and one yellow, in which the quantity of oxigen differed no more than one or two per cent. Proust, on the contrary, found no more than two oxides, of which he has determined the composition with considerable accuracy; and Bucholz, who repeated the experiments of Thenard with the intention of examining them, could find only two degrees of oxidation precisely the same which Proust had described. I have found as many as four, which it is incontestible that Thenard saw, though he did not well distinguish them from the mechanical mixtures of different degrees of oxidation, which are very frequently obtained; and though he has given no other distinctive character than the colour, which is so often fallacious.

The author has found four.

Antimony was purified by fusion with its oxide.

I must observe, that the antimony employed in all my experiments was purified in the following manner: I reduced it to powder, and mixed it with the white oxide of antimony, which I then exposed to fire till the mixture was fused. If the fused oxide, which flowed above the metallic bottom, was found to be coloured after cooling, I repeated the same operation.

1. Suboxidum stibicum; formed in the atmosphere; blackish grey.

1. *Suboxidum stibicum* is formed when the metal is exposed for a long time to the action of an humid and warm atmosphere. It forms an extremely thin coat of a blackish grey colour, which prevents all farther action of the atmosphere upon the parts so covered. In order to obtain this suboxide in

larger quantities, a piece of antimony, fused in a tube of glass, to give it a convenient form, was employed as the positive conductor in the decomposition of pure water by a voltaic pile of fifty pair. The antimony produced oxigen gas in extremely small bubbles, but, at the same time, it became covered with a grey pellicle which became almost black when the metal was dried in the air. That part of the antimony which was covered by the cork had preserved its metallic brilliancy, and the difference between the suboxided surface and clear metal, was very marked. But as even in this experiment the suboxide did not appear visibly to increase as soon as the pellicle was formed, I employed antimony reduced to powder as the positive conductor, and touched at the bottom by the point of the platina wire. This point produced oxigen gas, which, from time to time, rose through the powder, and this last began to be covered with a lighter and bluish powder. After some days this powder had increased so much as to be capable of being separated from the metal by means of levigation. This becomes nearly black by drying, and, when rubbed with a polished bloodstone, did not give the smallest trace of metallic brilliancy. When thrown into muriatic acid, this fluid emitted a slight smell of hydrogen, and a few instants afterwards, metallic particles were seen swimming in the acid, and were more easily precipitated by soda than the powder before the action of the acid. The suboxide of antimony, therefore, possesses a property common to most of the suboxides, of being decomposed by the action of acids, by concentrating the oxigen upon part of the metal to produce a base combinable with the acid, and reducing the other part to the metallic state.

Also by voltaism.

Another voltaic process on the antim. powder.

Decompos. of the suboxide by an acid.

I have not been able to produce this suboxide in a sufficient quantity to analyse it; but I shall hereafter shew how it is possible to find its composition by calculation with some degree of probability.

Composition not determined by the exp.

2. *Oxidum stibiosum.* The characters of this are very well known from the experiments of Proust and Thenard. It has a dirty white colour, is slightly soluble in water, is easily fused by a cherry red heat into a yellowish fluid. The mass, when cold, is crystallized in the manner of asbestos, but the groups of crystals cross in every direction, and it is not difficult to break their continuity.

2. Oxidum stibiosum. dull white, fusible, &c.

a. In order to determine the quantities in the composition of this oxide, I digested ten grammes of antimony with nitric acid until they were completely corroded. I then mixed the liquid with much water, and washed the precipitate with water until that fluid came off without being capable of reddening turnsole. The oxide thus obtained weighed, when dry, 12·065 grammes; and, in order to drive off all water, I exposed it in a glass capsule to an heat not as high as ignition, but it took fire on a sudden, and continued to burn like fungus, (or tinder) at the same time subliming in a thick white smoke, of which part was condensed on the sides of the glass. The powder, by this means, became as white as snow, and weighed 12·3 grammes.

Formed with antimony corroded by nitric acid.

Takes fire by low heat, and becomes very white.

b. As this analytical method did not appear very good, I mixed in a small glass retort ten grammes of murias hydrargyricus (corrosive sublimate) in powder, with twenty grammes of powdered antimony. The atmospheric air of the retort having been expelled by hydrogen gas, and a small receiver filled with hydrogen gas being also applied, I gently heated the mixture until the murias stibiosus (butter of antimony) came over, and lastly I heated the body of the retort red hot, to distil over the mercury amalgamated with that part of the antimony which had been added in excess. The quantity of 16,98 grammes of antimony remained in the retort. Consequently ten grammes of murias hydrargyricus had been decomposed by 3·02 grammes of antimony. But 100 parts of this salt contain 5·75 of oxigen combinable with other metals. 100 parts of antimony had, therefore, been combined with thirteen parts of oxigen.

Another method by heating antim. and corros. sublimate in hidrogen;

and from the quantity of the latter decomposed by a known quantity of antim. the components of the oxide were deduced, viz. 100 parts antim. and about 19⅜ oxigeu.

I repeated this experiment several times without having ever obtained results perfectly equal, but varying, as for example, 19,35 or 19,68 parts of oxigen for 100 of antimony The causes of error in this experiment may be several. For instance, it is possible that the mercury may be so adherent to the antimony as not to be separated but at a temperature which would also carry over a little of the latter; and it is also possible that a small quantity of mercurial muriate may arise before decomposition along with the vapors of the muriate of antimony. It is, therefore, probable, that these experiments may have given the quantity of oxigen rather too great.

Indirect method of deducing the components of an oxide.

In my essays on determinate proportions, I have often ascertained the composition of an oxide, which was difficult to analyse with exactness, by analysing the sulphuret of the same metal, and calculating the composition of the oxide from this analysis. I endeavoured to do this in the present case.

Sulphuret of antimony was made by heating antimony and cinnabar.

Cause of inaccuracy.

Inference that 18·6 parts oxigen and 100 antimony form this oxide.

3. *Sulphuretum stibii.* I mixed 100 parts of pulverized antimony with 500 parts of very pure cinnabar, and I exposed the mixture to heat in a retort. When the cinnabar appeared to be entirely decomposed, and the excess driven out of the bulb of the retort, I left the sulphuret of antimony in fusion for several minutes at a cherry red heat, and then took the retort from the fire. The sulphuret of antimony weighed 137·3 grs. In the upper part of the retort I found a small quantity of a reddish substance sublimed. I supposed it to be cinnabar not completely expelled, and heated the sulphuret again in the retort till it boiled. The red substance was increased, and I at last discovered that it was crocus of antimony produced by the access of air. As the sulphuret of antimony is slightly volatile in a very elevated temperature, the result of this experiment likewise is not very exact; but it may, however, be inferred, that the quantity of oxigen in the oxidium stibiosum cannot be less than 18·6 for one hundred parts of metal.

4. White oxide. (*a*) Antim. oxided by nitr. ac. and ignited.

4. *White oxide of antimony*[*]. (*a*) Two parts of pulverized antimony oxided (in a phial carefully weighed) by pure nitric acid, and the oxided mass strongly ignited in the phial, produced in different experiments 125·8, 126:13, and 127·8 parts of white oxide.

(*b*.) Antim. dissolved in nitro-mur. acid, and precipitated by water and ignited.

Deduction, 100 antim. and about 26 oxigen, *which is nearly 1½ times the oxigen in the second oxide.*

b. 100 parts of pulverized antimony first dissolved in nitro-muriatic acid, and then precipitated and well washed with water, produced a quantity of oxide of antimony, which, after strong ignition, weighed 126·56 parts. The acid liquor, after dilution, contained no more oxide, and did not become turbid by saturation with an alkali. The experiments appear, therefore, to prove, that this oxide does not contain less than 25·8 nor more than 27·8 of oxigen to 100 parts of metal. We see, therefore, that this oxide must contain 1½ times as much oxigen as the preceding degree, though the exact number was not ascertained.

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[*] I shall hereafter explain why I do not here use the words *oxidum stibicum*.—B.

Exp. The oxide of the first degree was heated with the metal (antim.) the combination was fusible, &c.

In order to obtain a more determinate result by another process, I endeavoured to reduce the white oxide at the first degree of oxidation by means of metallic antimony. I, therefore, mixed the metal in extremely fine powder, with less of the oxide than would have been required to oxide the metal. I introduced the mixture into a small phial, of which I drew out the neck into the form of a capillary tube. The body was bedded in sand in a small crucible, and exposed to a sufficiently strong fire to make it red-hot; and, at the moment when the matter entered into fusion, I hermetically closed the end of the capillary neck, by melting the extremity, and I left the mixture in that heat for half an hour. Three grammes of the white oxide of antimony had oxided 0·323 grs. of metallic antimony, and afforded a fusible oxide. I reduced this again to powder, and mixed it with powdered antimony, after which I fused it in a similar phial; but in its present state it was capable of dissolving only a small quantity of antimony, which would have been correspondent with 0·03 grs. of antimony upon the whole quantity of oxide. The fused oxide which I had obtained by this operation, was of a pearl colour, its fracture crystalline, granulated, and very compact; it was extremely coherent, and difficult to break, and all its external properties proved that it was not a pure stibious oxide. I repeated this experiment several times, and always found that the white oxide, fused with metallic antimony, dissolved one-third more than it before contained; that is to say, that 100 parts of the oxide can oxide an addition of 26 parts of the metal; and if we attend to this result, which is determined with the utmost possible accuracy, we shall find that the fusible oxide produced, of which the external characters are so different from those of the oxidum stibiosum, cannot be the same as this last; but that it must be a combination of the oxidum stibiosum with the white oxide in such proportion that the oxigen of the former is double that of the latter. Such combinations between oxides of different degrees of the same metal, are not very rare, though they have not hitherto been much attended to, or else have been considered as different degrees of oxidation of the metal. I reduced the oxide into powder, and

100 parts oxide had oxided 26 more of metal,

forming a compound of two distinct oxides of different degrees.

and digested it with the surtartrate of potash: the stibious oxide was dissolved, while the white oxide was separated in the form of an extremely white and voluminous powder.

Exp. To shew the proportions of sulphur and of oxigen which can separately unite with a definite quantity of antimony. White oxide and sulphur were heated together.

In order to compare the quantity of oxigen in the white oxide with the quantity of sulphur which can combine with the metal it contains, I mixed 100 parts of white oxide with 100 parts of sulphur, and heated them together in a small phial exactly weighed, which had a narrow neck, and of which the aperture was closed with a stopper of charcoal. The heat employed was at first very gentle, until the disengagement of sulphureous acid had ceased, after which I heated the phial amidst the burning charcoal until the uncombined sulphur was entirely dissipated, and the buton of sulphuret remained in fusion at the bottom of the phial. It weighed 107·25 p. Although the volatility of the sulphuret of antimony could not be perfectly obviated in this experiment, I have reason to think, that the length and smallness of the neck of the phial prevented any loss, and more especially as the mass was never so much heated as to put it into a state of ebullition*.

(To be continued.)

III.

A Reply to Don JOSEPH RODRIGUEZ'S *Animadversions on Part of the Trigonometrical Survey of England.* *By* OLINTHUS GREGORY, LL. D. *of the Royal Military Academy, Woolwich.*

To W. Nicholson, Esq.

DEAR SIR,

Surprise on seeing an attack on the English na-

WHEN I say that I have been greatly surprised to see in the second part of the Philosophical Transactions for 1812, Don Rodriguez's animadversions upon part of the Eng-

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* I never operated upon a substance which in general afforded me results so various as antimony and its oxides. In order to ascertain whether the oxides, which are often obtained with less oxigen than they ought to contain, have been volatilized with the acid employed for their oxidation, I heated such an oxide with sulphur, and converted it into sulphuret. 100 p. of antimony produced 128·5 of yellow oxide, which by ignition left 125·8 of white oxide, and these produced with sulphur 137·5 of sulphuret. It appears, therefore, that these oxides contain combinations of different degrees of oxidation, which prevent the saturation with oxigen.

lish

tional survey in Phil. Trans.

lish Trigonometrical Survey, I conjecture that I am merely describing a feeling which has been more or less experienced by every man of science in the kingdom. The publication of an attempt by a *foreigner* to cast discredit upon a great national undertaking, in the transactions of the most eminent philosophical institution of that nation, the Royal Society,—that is, in a work which learned men on the continent contemplate as a fair picture of the science and genius of England, is, I believe, a thing unprecedented in the history of literature. If the great work which Don Rodriguez has taken upon himself to examine, had been really reprehensible, it would still have been extraordinary that he should have been permitted to give his censures currency in such a vehicle; but how much more extraordinary must it be thought, if, on inquiry, it shall appear, that his strictures are causeless, and therefore unjust. This is an inquiry which every man of competent information, who has at heart the honour of his country, has a right to institute; and, however unpleasant the undertaking may, in some respects be, I enter upon it without delay, because *Colonel Mudge*, whose reputation is so deeply implicated in this business, is at present prevented from giving Don Rodriguez's paper that decided and complete refutation which it will hereafter receive at his hands, and because his silence, though unavoidable, may be construed into defeat.

Dr. G.'s reasons for quoting authorities.

Impressed by these considerations, I propose, in this communication to show, that the observations of this ingenious foreigner are, on all his main positions, unfounded: and, although the matter under investigation is, in general, so nearly elementary that any man of moderate scientific attainments might safely rest the truth of his assertions upon his own character and their intrinsic evidence; yet, lest it should be apprehended that, on this occasion, my judgment may be warped either by strong national feeling, or by private attachment, I shall fortify my positions, as I go along, by such authorities as neither Don Rodriguez, nor any other person, will be inclined to question.

Before I proceed to the points which Don Rodriguez selects as the basis of his animadversions, it may not be thought improper if I briefly advert to what appears his main, if not his sole, object, in making those animadversions at all. I shall not

The Don's *apparent* object to exalt the French scientific character,

at the expence of all others.

not, I hope, be deemed uncandid, if I say, that to me this object *appears* to be no other than the depression of English (and perhaps other) ingenuity and exertion, in order to the undue exaltation of the French scientific character. To this end, as it would seem, (for to what other purpose can it be?) we are told, that in consequence of "the general impulse which the human mind received" from the French revolution, the members of their Academy of Sciences "invented new instruments, new methods, new formulæ," for the purpose of ascertaining the figure of the earth, &c. and commenced "an important undertaking, almost the whole of which consisted of something new in science." I have no wish to depreciate the value of the discoveries and improvements of the French mathematicians; yet surely I may affirm, that much had been done with respect to the grand topic in question, long before the French revolution. Did not *Euler* invent "new methods and new formulæ" for this express purpose, and publish them so long back as the year 1753, in the Berlin Memoirs? Did not *Dionis du Sejour* much improve this branch of analytical theory? Did not Professor *Playfair* solve the general problem in all its *useful* varieties in the Edinburgh Transactions, before the publication of Delambre's investigations? Did not *General Roy*, and the subsequent English measurers, publish ingenious formulæ in the Philosophical Transactions, although Don Rodriguez insinuates, that their methods are kept back? And, with respect to actual admeasurements, might not the Don have learnt from the Philosophical Transactions (see vols. 75, 77, 80, &c.) that government surveys were commenced in Scotland so long back as 1745, by Lieutenant-General Watson; that in 1775 the work was continued; that in 1783 an authorized committee or deputation of the mathematical philosophers of England and France met at Dover to concert the best means of carrying a series of triangles from Greenwich to Paris; that the work was soon after pursued by the appointed persons in both countries; and that, from that period it has almost regularly proceeded in England, whatever interruptions it may have experienced in France? How, then, can a writer insert in the Philosophical Transactions, where evidence to the contrary *abounds*, a paper from which all who are unacquainted with the history of this important class of operations, would conclude

(See also Nich. Journ. New Series, Nos. 26, 27, or vol. VII.)

English Trig. Survey commenced before the French revol.

conclude, that they *originated* in the determination of the French to "establish a new system of weights and measures."

To the same end, apparently, tends the Don's assertion, that "the Swedish Academy of Sciences, *encouraged by the success of the operations conducted in France*, sent also three of its members into Lapland to verify *their* former measurement." For the natural tendency of this statement is to produce the belief, that the recent operations of the Swedish philosophers were in humble imitation of the French, and that they were undertaken for the purpose of verifying, or of correcting, *their own* former admeasurement; in both which respects the colouring given is widely different from the truth. The Lapland measure in 1736 was not conducted by Swedish, but by *French* academicians; and the correction of it was proposed long before the French revolution. The following are the true circumstances of the case, as I received them from a learned Swede. *Melanderhielm*, the venerable president of the Stockholm academy, had almost from his youth doubted the accuracy of the operations of 1736, and sought anxiously for an opportunity of repeating them; but waited many years before he could avail himself of a favourable conjuncture of circumstance, although latterly he had found in M. *Svanberg*, a *young* man of great talent and activity, to conduct the operative part. After hearing of the new measure of a degree by MM. *Delambre* and *Mechain*, he wrote to some of the French mathematicians on the subject, but with no intention of soliciting them to visit Lapland. Soon after this, Buonaparte, at the suggestion of the then national institute, *wrote* a letter personally to the late king of Sweden, requesting permission for some members of that body to proceed to Lapland, in order to determine an arc of the meridian. That high-spirited young monarch replied, that he would consult his own Academy of Sciences at Stockholm, whether such an operation was desirable for the interests of science; and if they were of that opinion, he had no doubt he could find *Swedish* mathematicians competent to the undertaking. Hence MM. *Svanberg, Ofverbom, Holmquist, and Palander*, were appointed to examine and repeat the measure of the French academicians; and this is what Don Rodriguez terms the expedition of *three* of the Swedish academicians "to Lapland to verify *their* former measurement."

History of the New Lapland measure;

and of Buonaparte's letter to the king of Sweden.

Col. M. commended by Don. R. *merely* as a skilful observer.

With the same spirit, it is natural to suspect, Don Rodriguez speaks of Colonel Mudge as "*a skilful observer*," and merely such; adding, that "one cannot but admire the beauty and perfection of the *instruments* employed" by him: while, when he characterises the labours of the French measurers, he assures us they "merit the *highest* degree of confidence," and, "by the sanction of such an union of talents, give such a degree of credit and authenticity to *their* conclusions, as could scarcely be acquired by other means." I shall not animadvert upon this invidious contrast; but simply remark here, that the Don adopts a strange method of verifying his positions. He admits, that Colonel Mudge is a *skilful observer*, who knows very well how to employ his instruments; and, that there may remain no doubt on that head, publishes a long paper to prove, or at least to show it probable, that he has made a *mistake* of 4½ seconds in the determination of a zenith-distance. This animadverter has, as he assures us, gone through *all* the Colonel's computations by different processes, and found them correct, or only evincing very trifling discrepancies, such as may naturally arise from the diversity of methods; yet he cannot find in his heart to drop a single word of commendation on him as a computer or as an investigator.

The preceding remarks will suffice, I apprehend, to render manifest the probable object of Don Rodriguez's paper. I shall now proceed to enquire how far the reasons assigned by this gentlemen bear him out in his attempt to throw suspicion upon the operations of Colonel Mudge, in measuring an arc of the meridian. The Don's paper, it is true, is rather desultory and unconnected; but, I trust I shall neither misrepresent him, nor do injustice to his arguments, by endeavouring to reduce them to the following order:

Don. R.'s first reason for questioning the accuracy of the English observations refuted.

1. Colonel Mudge's observations must be wrong somewhere, because his results do not correspond with those of the French measurers. This is not positively affirmed, but every where strongly implied: for Don R. assumes his value of the radius of the earth's equator from the French measurements and computations; and he takes it for granted, that the fraction exhibiting the ratio of the difference of the earth's axes to the major axis, technically termed the *compression*, lies somewhere between those limits, ($\frac{1}{330}$ and $\frac{1}{310}$,) which a superficial ob-

server would adopt as most suitable to the French operations. Such assumptions, by the way, are neither consistent with fair criticism nor with sound logic: for the grand object in measuring arcs of meridians is to DETERMINE *the ratio* of the earth's axes; and when, in the course of any such admeasurements, avowedly remarkable anomalies arise, it is a mere *petitio principii* to conclude that there *must* be some error in the astronomical *observations*, because irregularities as great or greater than those which the operations indicated, result from computations resting upon a gratuitously assumed ratio.

The French operations indicate *greater* irregularities.

But some of the French operations at home, compared with those at Peru, give about $\frac{1}{309}$ for the compression*. Be it so. That is no reason why any such ratio should be adopted, as the test by which to try the accuracy of English *observations*. Don Rodriguez himself, when applying the same test to the French meridian, thereby detects irregularities, and great ones too; yet does not whisper the gentlest hint that they were occasioned by inaccurate observations. Why not? Because M. Mechain "handled instruments with *great* delicacy, and was possessed of *peculiar* talents for this species of observation." So that a gratuitous assumption should suffice to render English observations doubtful, while it leaves the accuracy of French ones unimpeached. To me it appears that a candid critic would, in analogous circumstances, make analogous inferences; and not sift one class of results to the bottom, while he satisfies himself with merely glancing at the surface of the other class. Had he examined the French measures a little more minutely, he would, instead of adopting them as his standard, have found that they exhibit far too great irregularities to be entitled to that honour. Taking the results of the operations of Delambre and Mechain, as subdivided naturally by the assumed stations at Dunkirk, the Pantheon at Paris, Evaux, Carcassone, and Montjouy, and applying to them the principle developed by Legendre, in which, "the sum of the *squares* of the errors is made a *minimum*," the requisite compression is $\frac{1}{148}$; and even then the deviations from what the theory would require are, at Dunkirk $-2''\cdot23$, that is, nearly $2\frac{1}{4}$ decimal seconds; at the Pantheon, $+5''\cdot63$; at Evaux, $-4''\cdot79$; and at Carcassone,

Some anomalies in the French measures.

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* Biot. Astronomie Physique, tom. i. p. 159.

+ 1″·34. Here the compression which agrees best with the observations is more than *double* what it ought to be. If a medium compression had been chosen, the errors at the several stations would have deviated still farther from the probable errors of observation. Don Rodriguez will find this confirmed by Puissant, *Géodésie*, pa. 137. 141, and by Laplace, *Exposition du Systême du Monde*, Liv. i. ch. 12. After he has duly reflected upon the deductions of those philosophers, he will, perhaps, be convinced, that he has been rather precipitate in taking the French operations as a standard.

Don R.'s 2d reason refuted.

But, secondly, this writer infers that there must be some error in Colonel Mudge's observations, because they tend to shew that the terrestrial spheroid is very irregular. All the measurements "which have been hitherto made in the northern hemisphere, are (he tells us) *extremely satisfactory by their agreement*, and give us great reason to presume, that the general level of the earth's surface is elliptical and *very regularly so*." "There would not have remained the smallest *doubt* respecting the earth being flattened at the poles," but for the "measurement performed in England." But "this measure alone would lead to the supposition, that the earth, instead of being flattened at the poles, is, in fact, more elevated at that part (the author means *those parts*) than at the equator, or at least, that its surface is *not that of a regular solid*." The degrees, in fact, increase as the latitudes diminish, which, says Don Rodriguez, "excites a suspicion of some incorrectness in the observations themselves;" whereas, the only fair inference is, that an *insular* situation is very ill fitted to promote the determination of the figure of the earth. Let us see, however, how "*satisfactory*" former measures have been "*by their agreement*," and how completely they prove that the earth's surface is "*very regularly*" elliptical. Lacaille's degree in lat. 45° N. compared with Bouguer's at the equator, gives for the compression $\frac{1}{218}$. The degree in Maryland, with Bouguer's equatorial, gives $\frac{1}{506}$. The Spanish degree at the equator, with the French degree lat. 45°, gives $\frac{1}{503}$. Boscovich's Italian degree, lat. 43°, compared with Bouguer's at the equator, gives $\frac{1}{303}$. Bishop Horsley, by a geometrical mean of twelve different ellipticities, obtains $\frac{1}{208 \cdot 7}$. Boscovich, taking a mean from all the measures of degrees, so as

Former measures do not agree in their results, as Don R. affirms.

Summary of statements,

as to make the positive and negative errors equal, obtains $\frac{1}{248}$. Lalande, by comparing Father Leisganig's degrees in Germany with eight others in different latitudes, gets $\frac{1}{311}$. And the recent measures in France give, as we have seen, $\frac{1}{148}$. Such is a summary of the evidence from which it is to be concluded that the earth is "elliptical," even "very regularly so." General Roy, who had got a habit, not very uncommon among scientific Englishmen, of deducing reasonable conclusions from anomalous appearances, and not twisting them to suit a fanciful hypothesis; assumed *seven* different spheroids of varying ratios between $\frac{1}{170}$ and $\frac{1}{540}$, and, on finding that none of them corresponded so uniformly as might be wished, with the operations in different latitudes, made these inferences: "Hence it is obvious, that the arcs of an ellipsoid, however great or small the degree of its oblateness may be, will not *any way correspond* with the measured portions of the surface of the earth." "Hence it is that philosophers are not yet agreed in opinion with regard to the figure of the earth; some contending, that it has *no regular figure*, that is, not such as would be generated by the revolution of a curve around its axis." And again, after specifying some other facts, "from all which we may conclude, that the earth is *not an ellipsoid*."

and General Roy's deductions from them.

Puissant's deductions

Nor is this opinion peculiar to General Roy, it is common, I believe, to all who have contemplated the subject, except Don Rodriguez. Thus, Puissant, at p. 187, of his *Géodésie*, says "La comparaison des divers degrés mesurés à l'équateur, en France, en Pensylvanie, etc. donne lieu à décider que les méridiens sont différens entreux et n'ont pas la forme elliptique." And at p. 222. "D'on l'on doit conclure que la terre *n'a point* la forme régulière que l'on serait tenté de lué attribuer." To the same purpose writes Laplace, at p. 56 of his "*Exposition*:" "Les degrés du nord et de France donnent $\frac{1}{146}$ pour l'ellipticité de la terre, que les degrés de France et de l'équateur, donnent égale à $\frac{1}{334}$, il paroit donc que la terre est sensiblement différente d'un ellipsoïde. Il y a même lieu de croire *qu'elle n'est pas un solide de révolution*, et que ses deux hémisphères ne sont pas semblables de chaque côté de l'équateur.

Laplace's

It is curious, however, to observe that, notwithstanding this extreme want of uniformity, in the results furnished by terrestrial

trial admeasurements, those which are deduced from astronomical theory, and the oscillations of pendulums, correspond very nearly. Thus, Laplace's deduction of the compression from the lengths of pendulums in different latitudes, is $\frac{1}{335.78}$. (See Puissant, *Topographie*, &c. p. 66.) Clairault's well known modification of Newton's theorem, derived from the diminution of gravity, gives $\frac{1}{304}$. The phenomena of the precession of the equinoxes and the nutation of the earth's axes, give $\frac{1}{304}$ for the maximum limit. A lunar inequality in longitude depending upon the earth's ellipticity, and expressed by $-20''\cdot 987$ sin. ☊ of the moon in longitude, requires the compression to be between $\frac{1}{314}$ and $\frac{1}{305.05}$, but nearest the latter limit. And a lunar inequality in latitude, depending also on the compression, and expressed by $-24''\, 6914$ sin. ☽, requires the compression to be between $\frac{1}{334}$ and $\frac{1}{304.6}$, still leaning to the latter limit. So that the ratio of the earth's axes, as deducible from these independent theoretical considerations, lies within much narrower limits than we can get in any other way. But this does not affect the truth of the preceding remarks. It serves principally to shew, that whatever may have been the derangements of the terrestrial spheroid since its original formation, they are not such as have differently affected the several phenomena occasioned by its aggregate attraction: while a very slight consideration of the effects of the deluge, of earthquakes, of volcanic operations, of extensive dislocations of strata, &c. may serve to convince us, that, however regular the earth might once have been in its general shape, there is now no reason to expect that "very regular" surface from which Don Rodriguez persuades himself there ought to be no essential deviation.

The only inferences which agree are those from pendulums, and astronomical theory.

Remarks on this fact.

A 3d reason of the Don's refuted.

3. Don Rodriguez is farther confirmed in his opinion, that there must be an error in the observations, especially at Arbury Hill, of "nearly 5 seconds," because he thinks no such anomaly as that can fairly be ascribed to the effect of local attractions. He does not deny "that irregularities of the earth and local attractions may occasion considerable discrepancies;" yet he does not believe they can ever produce a deviation of the magnitude just specified. Here again he is at war with the decisions, I believe, of all preceding philosophers who have directed

Three causes of a deflection of the plumb-line.

directed their attention to this subject. There are, obviously, three causes which may jointly or separately occasion a deflection of the plumb-line from the true perpendicular to the earth's surface; namely, an insular situation, the attraction of mountains, and strata of unequal density beneath the surface: and either of these may be productive of considerable effects.

To arrive in the easiest manner at an estimate of the effect upon a plumb-line arising from observations made in an insular situation, let Don Rodriguez imagine the simple case of a triangular island so posited on the surface of an aqueous spheroid, that a meridian shall run along from its vertex, directed northward to the middle of its base: he will perceive that, in such a case, as an observer proceeded from the south towards the north, there would be a constant variation in the deflection of the plumb-line; in such manner, that there would be only one point on the meridian, where the attractions occasioned by the island itself should be so counterpoised and adjusted, that the true and observed vertical lines should correspond. Pursuing this hypothesis, with the requisite modifications for a neighbouring continent on the south, and an immense ocean north, he will find that the singular order exhibited by the English estimates of degrees, though an unexpected, is by no means an unnatural, consequence of our insular situation. Dr. Hutton has treated this very point with his usual perspicuity, in a valuable note at page 198, vol. ii. *New Abridgment of the Philosophical Transactions*, published in 1803. That note is too long to be copied into this place; I shall, therefore, merely transcribe the Doctor's concluding inference: "Hence also it follows, that insular situations must be worst of any; having the plumb-line deviating to the north at the south end of the line, to the south at the north end, to the east at the west side, and to the west at the east side; thus producing errors in all observed latitudes and longitudes."

Dr. Hutton on the deflection from insular situations.

Laplace ditto.

Laplace, most probably alludes to this kind of effect, at p. 59, "Exposition," where he speaks of the *much more extensive attractions than those of mountains*, of which the effect is sensible in Italy, *England*, &c.

That the deflections of the plumb-line, and the consequent estimate of the lengths of degrees, must be greatly affected by hills and valleys, is also very manifest. Professor Playfair, after

Playfair on the attraction of hills, &c.

after describing the irregularities thus occasioned in the degree at Turin, adds, "there are, *no doubt*, situations in which the measurement of a small arch might, from a similar cause, give the radius of curvature of the meridian, *infinite, or even negative.*" See *Edinburgh Transactions*, vol. v. p. 5. And Dr. Maskelyne, after treating of Mason and Dixon's degree in North America, says, "Mr. Henry Cavendish having investigated several rules for finding the attraction of the inequalities of the earth, has, upon probable suppositions of the distance and height of the Allegary mountains, from the degree measured, and the depth and declivity of the Atlantic ocean, computed what alteration might be so produced in the length of the degree; and finds that it may have been *diminished by* 60 *or* 100 *toises* by these causes. He has also found, by similar calculations, that the degrees measured in Italy, and at the Cape of Good Hope, may be *very sensibly* affected by the attraction of hills, and defect of the attraction in the Mediterranean Sea and Indian Ocean." *Phil. Trans.* vol. lviii. or *New Abridgment*, vol. xii. p. 578.

Maskelyne.

Cavendish.

Puissant on local attractions.

Playfair on ditto.

With respect to the third cause of irregularity, Puissant, Géodésie, p. 137, remarks, that "anomalies in the latitudes, are, *doubtless*, produced by local attractions which change the direction of the apparent vertical." And Professor Playfair, in the excellent memoir I have just quoted, (a memoir, it should be recollected, which was written *five* years before the remarkable anomalies in the English measures were known) affirms, that "from suppositions no way improbable, concerning the density and extent of masses of varying strata beneath the surface, he has found, that the errors thus produced, *may easily amount to ten or twelve seconds.*" "This cause of error, (as he justly remarks) is formidable, not only because it may go to a great extent, but *because there is not any visible mark by which its existence may always be distinguished.*"

Here, then, are *three* sources of deflection from the true plumb-line, neither of which is correctly appreciable in all circumstances, yet of which each may be not only perceptible, but important; and the concurrent effect of all may, doubtless, be very considerable. Yet, Don Rodriguez is unwilling to attribute a deviation of 4 or 5 seconds, to any, or all, of these causes.

4. This writer infers, that mistakes must have occurred in the observations, because the sum of other "errors will be found in the estimate of the entire arch, and will increase in proportion to the extent of the arc measured; but in the English measurement, we find exactly the reverse of this." Here he assumes the principle proposed by Boscovich, but condemned by Laplace, for a reason thus briefly assigned by Puissant:—"La solution donnée d'abord par Boscovich est *vicieuse* en ce qu'elle est fondée sur une *hypothèse inadmissible,* savoir, que les erreurs dans le mesure des arcs du meridien sont proportionelles à leurs longueurs."

Don R.'s 4th reason contradicted by Laplace.

5. He concludes that there must be "an error of some seconds in the observations of the fixed stars," because "the results of the observations made on different stars, differ no less than 4 seconds from each other." Now, what are the facts on which this inference rests? Simply these: that the only two stars which indicate any such difference in the whole series of observations, are μ Draconis and ζ Ursæ; that they give a difference of 4″·19, not in the amplitude of the arc between *Dunnose and Arbury Hill,* but of that between *Dunnose and Clifton*; and that, whether these two stars be rejected, or retained with the other *fifteen* employed in finding that amplitude, they will not occasion a difference of a quarter of a second in the result. How, then, can a fair investigation bring this as a reason for an alleged inaccuracy, when it obviously cannot apply to the case? And what must be thought of his impartiality, if it shall appear, that *even in this respect,* the observations of the French and of Major Lambton, which he so manifestly prefers to the English observations, are far more open to censure? Allow me, therefore, just to make the comparison.

Don R.'s 5th reason of no weight; because the stars, where he finds the irregularity do not apply to *Arbury,* but to Clifton.

Of the English observations, *none* are suppressed, (the observers going upon the principle explained by Simpson, in his "Tracts," which clearly establishes the propriety, if not the necessity, of taking the mean of a *number* of observations) and yet, no irregularity of consequence, except the one above specified, appears. But, it may be seen from p. 72, *Discours Preliminaire,* tome, i. *Base du Système Métrique Décimal,* that no less than *sixty-eight* of the French observations upon β Ursæ majoris were rejected, and termed *bad*; for no other reason

Much greater irregularities in the French observations; they evince, besides, a little "*coaxing.*"

reason that I can perceive, than, that if they had been employed, they would have given the latitude of Dunkirk about a second less than the observations of the pole star gave it. Let Don Rodriguez reflect upon this, and then repeat that the French operations "*merit the highest degree of confidence.*" But this is not all. From p. 39 of the same *Discours Preliminaire*, it appears that *three* stars only were *selected* by Mechain at Montjouy, in consequence of the coincidence of the results arising from them. Among the stars rejected, was ζ *Ursæ*, because different observations gave a difference of 4″. So that the French also detected an irregularity respecting this star.

Herschel's observations on double stars, show the reason of the apparent anomaly, in Col. M.'s case, and make it rather a proof of accuracy.

They assign, however, a wrong reason for the fact; for they attribute it to errors in Bradley's table of refractions, while the truth is, that ζ Ursæ is a *double* star, by no means easy to observe properly. Indeed, it appears, not only from the observations of Col. Mudge, &c. but from those of Dr. Herschel (*Phil. Trans.* vol. lxxxii. *New Abridgment*, vol. xv.), that both μ Draconis and ζ Ursæ are double stars; that of the former, the two constituent stars appear equal, both white, and not easily distinguishable, and at the distance of 4″. 35 from each other, mean measure; and that of the latter, the two are considerably unequal, and the largest difficult to bisect. Hence Herschel's observations completely confirm those of our trigonometrical surveyors. See also the catalogues of Wollaston and Bode.

Major Lambton's observations so warmly commended by Don R.; abound in *great* discrepancies.

Let us next enquire how far Major Lambton's observations, which Don Rodriguez also seems to delight in eulogizing, deserve to be preferred to Colonel Mudge's. From p. 356, vol. x. *Asiatic Researches*, we learn that the Major's observations upon α serpentis were 14, of which two were 5°57′ 3″·38 and 5° 56′ 53″·98 furnishing a difference of 9″·3; more than *double* the difference that has been found in the English observations, of which the Don complains! At p. 357, again we have a register of 16 observations upon γ Aquilæ, of which two differ by 6″·77. At p. 358, we have 18 observations upon Atair, of which two differ by 5″·38. There are also some other palpable differences in Major Lambton's results, as deduced from *different* stars. The greatest is between Atair and Markab, being 5″ 48. Atair, from the number and agreement of its observations among themselves, should be correct in zenith distance, yet it gives the latitude of the station, Dodàgoontah, less by 3''·4 than the mean of the nine stars, employed by Major Lambton, exhibits it, and the latitude found from a mean of the four northern stars, is 2'' 04 *greater* than the latitude found from a mean of the five southern stars. Discrepancies of more than 4'' may likewise be frequently found in the observations recorded in vol. viii. of the "Researches." Most of them are, probably, in great measure, attributable to the imperfections in Major Lambton's sector, which is only of 5 feet radius (while the English is of 8 feet); and is provided with but few comparatively of the requisite means of adjustment: but whether they are to be ascribed to the observer or his instruments, they prove that Don Rodriguez has been rather precipitate in saying, "the same Major Lambton, who has *succeeded so well* in Asia, and is in possession of such *perfect instruments* for the purpose, would be *singularly qualified* for a similar undertaking in Africa." In matters which admit of examination and proof. it is not the custom with Englishmen to bow at once to the authority of a mere *ipse dixit.* Was Don Rodriguez really ignorant that, with respect to accuracy of observation, the English proceedings are thus greatly superior to those of the French and of Major Lambton? If so, how greatly is he to be pitied for writing so much on a subject he had previously so little considered. If he *was* aware of this superiority, how much more is he to be pitied, for giving so unfair and unnatural a representation of the business before him.

Why did not the Don recur to the ready test of repeating the observations at Arbury?

From one or other of the reasons I have thus examined, Don Rodriguez says, "*it is almost beyond a doubt* that it is to errors in the *observations* of latitude," the singularity in Col. Mudge's results must be ascribed. There *must* be an error of some seconds in the observations, "especially at Arbury Hill." And he asks, "How is this to be discovered?" How? Why, by simply *repeating* the observations at Arbury Hill. The position of the station is so clearly described in the Philosophical Transactions, that any person may find it within 20 feet; and the farmer who owns the field, can show the identical spot. Don Rodriguez, or some one of his friends, has, doubtless, *handy* circular instruments of the French construction, by which the zenith distances could readily have been taken, and then the correctness or incorrectness of the English observers might have been proved in a way from which there could be no appeal. Though, to be sure, if that plan *had* been adopted, and the English results had, in consequence, been verified, Don Rodriguez's paper could never have appeared.

There is, however, a method of determining the point, even without taking this trouble. Having then shown, I trust satisfactorily, that Don Rodriguez's reasons for imputing an error of 4 or 5 seconds to the English observations, are nugatory; I shall now proceed, with all possible conciseness, to show that *there cannot be an error of one second* either in the observations at Arbury Hill, or at Dunnose; and those at Clifton are, by the Don's own concessions, out of the question.

The mode of *fixing* the zenith sector precludes error.

First, the manner of *fixing* the zenith sector could not lead to error; for, "to procure for the external stand (says Col. Mudge, Phil. Trans. 1803) and thence for the whole apparatus, a firm foundation, I caused four long stakes to be driven into the ground, one for each foot of the stand, to which its feet were firmly screwed down. The surfaces of the stakes were cut off smooth, and brought into the same horizontal plane, by which means the interior frame and sector were placed much within the limits of their several adjustments." The whole was enclosed in a suitable observatory.

French observations at Chatillon made during a *high* wind, on a stage so tottering, that even a little breeze much disturbed the observers.

Don Rodriguez may perhaps think the French method of fixing their instruments, on some occasions, preferable to this. The reader shall judge. Their instruments, both for taking horizontal and vertical angles, were sometimes placed on *tottering* stages, so as to give anomalies in the angles from $5''$ to $8''$; furnishing, as Delambre terms them, "le tourment des observateurs." Thus, at p. 46, *Discours Preliminaire*, we are told that at Chatillon, there was a high wooden stage erected for an observatory, in which the carpenter had so badly done his work, that "le *moindre* vent agitoit toute la machine, de manière non seulement à rendre les observations moins sûres, mais *à inquieter les* observateurs." And on turning to p. 174, tome i. it will be seen that the observers had not to contend with a gentle gale; for they there tell us of the "*Grand vent* qui agitoit le signal et l'instrument." The whole was *blown down* shortly after. Will Don Rodriguez place reliance on observations made from such a platform in such a wind; and, notwithstanding, doubt the observations made with a stable instrument by the English? And let him not forget, that whatever error was thus occasioned in the distance between Boiscommun and Chatillon, is more than doubled in all the remaining triangles of the series, by reason of the bad shape of the triangle, Chatillon, Boiscommun, Chateauneuf.

No error can fairly be ascribed to the *construction* of the sector;

If no error in the English observations can be fairly imputed to the manner of *fixing* the zenith sector, neither can any be ascribed to the "*construction*" of the instrument itself. This was most positively declared by two very excellent judges, the late *astronomer royal*, and the Hon. *Henry Cavendish*, on their close examination of the instrument. It will also be inferred, without hesitation, by all competent judges, on reading the description of it in the Phil. Trans. for 1803. To those who have seen neither the instrument nor the description, it may suffice, if I remark, that the equality of the divisions on the arch, is evinced from this consideration, that on running the micrometer screw from division to division, over the whole arch, there was no where an indication of an error amounting to half a second; and that the instrument still continues free from important "derangement," is tolerably well proved by this, that the line of collimation has been *constant* during all the observations and all the journeyings of the sector, and that it still continues the same.

or to subsequent derangement;

or to a deviation from vertical position;

In the next place, it may be remarked, that no error in observation can be imputed to a deviation from "*vertical* position" in the sector. Important inaccuracy, in this respect, is precluded by the great length of the axis, by which the instrument is rectified; and by the ready and certain means of placing the plumb-line directly over the illuminated *dot* which marks the middle of the axis, or true centre of the divided arch. For want of these admirable modes of correction, all previous instruments are necessarily imperfect. It appears from Phil. Trans. for 1803, pp. 405, 406, that when the instrument is adjusted in one position by means of the plumb-line and dot, it is turned to a position at right angles to the former, and the adjustment confirmed; and this being the case in these two situations, the instrument must necessarily be vertical in all others.

Various reasons may be assigned to show that the sector could not, or to a deviation from the plane of the meridian;

not, at any of the stations, be out of the plane of the *meridian*. I shall select only two or three. As 1st, if the sector were inclined to that plane, just so much would the path of any star, in its apparent motion, be inclined to the horizontal wire of the telescope; instead of which, both Colonel Mudge and Captain Colby assure me, that when a star came into contact with the wire, the light of the star would appear on both sides of the wire for about three-fourths of a minute of time, the light on each side being equal at the central wire: which of itself is a positive proof. But, 2dly. had the sector been out of the plane of the meridian, the times of the transits of the extreme stars employed, as compared with two excellent time-keepers, must have shewn it. Farther, the errors arising from a wrong plane of the meridian, being comparatively very great in the extreme stars, and small in those near the zenith, it would follow that the error in Capella, which is almost at the extremity of the arch, would be great, compared with those in β. Draconis, κ Cygni, &c. which were within a small distance of the zenith. But the amplitude of the arch, between Dunnose and Arbury Hill, as derived from Capella, is 1° 36′ 20.″02, while those derived from the other two stars, are 1° 36′ 19.″42, and 1° 36′ 19.″94: a coincidence which proves that the instrument could not possibly have any perceptible deviation from the plane of the meridian at either station. Other reasons for coming to the same conclusion will appear, on attending to the precautions in adjusting by double azimuths, &c. as described in the Phil. Transactions.

or to its use in any way.

The correct position of the sector in *all* respects is further proved from this: that the observations, however distant in point of time, when the proper corrections for aberration, nutation, &c. are applied to them, reduce always very nearly to the same mean place.

Hence, it must be obvious, that no error could arise, as Don Rodriguez suspects, from the *instrument*, whether in "vertical position, construction, or some accidental derangement." I shall now advance still farther, and prove *that there is* NO ERROR *in fact*. For if there were any error in the zenith distances at Arbury Hill, it would at once be detected on comparison with the observations at Blenheim. Now, the distance between the parallels of latitude of Blenheim and Arbury,

There cannot be an error even of *half a second* at either Arbury or

139,822

139,822 feet, furnished by the survey, gives for the corresponding celestial arch, 22′ 59·″33, while the observations of γ Draconis at Blenheim, compared with the observations upon the same star at Arbury Hill, give 22′ 59·″6. So that there *cannot possibly* be an error of half a second at Arbury Hill, unless the observations, for five successive years at Blenheim, were all wrong: and Blenheim observatory, be it recollected, has been long celebrated for the excellency of its instruments; and is selected even by *Svanberg* for the accuracy of the observations there made.—So, again, with regard to the Dunnose station, the latitude of Portsmouth observatory, as inferred from the said station, and the data in the Trigonometrical Survey, is 50° 48′ 2·″65; while the Requisite Tables, the edition of 1781, give it 50° 48′ 3·″. So that the observations at Dunnose *cannot possibly* err half a second, unless there was an error made by Witchell and Bayley, in determining the latitude of Portsmouth observatory, with an admirable mural quadrant, by Bird. These two deductions, then, completely exclude sensible error at Dunnose and Arbury Hill: and these inferences, it is evident, might as easily have been made by Don Rodriguez as by me.

Dunnose, unless there be errors of the same, or greater magnitude, in the observatories at Blenheim and Portsmouth.

This gentleman may find still farther confirmation of the truth of the whole survey, if he will examine the operations by which the meridian of Dunnose is extended to Burleigh Moor, and those for carrying on a new meridian from Black Down to Delamere Forest. These, it is true, are not to be found (for what reason I cannot say) in the Philosophical Transactions. But they may be seen in the third volume of the Trigonometrical Survey, published in 1811, by order of the Board of Ordnance; a volume with which some of Don R.'s friends in England are doubtless acquainted.

As a last corroboration of the whole portion from Dunnose to Clifton, amounting to 2° 50′ 23″38; let me add, that when compared with the meridional arch of 3° 7′ 1″ at Peru, by means of the valuable theorem, investigated by Professor Playfair, (Edinburgh Transac. vol. v. pp. 8, 9.) for the comparison of *large arcs*; it produces $\frac{1}{332\cdot68}$ for the resulting *compression*. While Svanberg (pa. 192, "*Exposition*") gives $\frac{1}{331\cdot448}$ for the compression, as deducible from a comparison of *his* measure with that at Peru.

Confirmation of the whole arch, furnished by Professor Playfair's formula, for the comparison of large arches; the only *fair* mean of inferring the ratio of the earth's axes, from terrestrial measures.

Thus, we have confirmation upon confirmation, of the correctness of Colonel Mudge's operations, both general and particular; and of the extreme rashness with which Don Rodriguez has affirmed, that "*it is very* evident that the zenith distances of stars taken at Arbury Hill are affected by *some considerable error*." The matter in question might, as you will perceive, have been settled in narrower compass; but the celebrity of the institution under whose auspices the Don's animadversions are circulated, seemed, in some measure, to call for a tolerably full reply to his paper.

For the reply here presented, the public must consider me alone as responsible: and I trust that when the two papers have been compared, I shall not be thought to speak incompatibly with the courtesy due to a foreigner, or the respect due to a brother mathematician, when I say that Don Rodriguez has *completely failed* to establish the point, respecting which he ought to have felt *certain* before he commenced his strictures.

OLINTHUS GREGORY.

Royal Military Academy,
Woolwich, March 5th, 1813.

IV.

On the Existence of combined Water in muriatic acid Gas. By J. Murray, *Lecturer on Chemistry, &c. Edinburgh.*

To Mr. Nicholson.

Edinburgh, March 3, 1813.

SIR,

Late experiment of Sir H. Davy.

IN your Journal for January, an account is given of an experiment performed by Sir Humphry Davy in the College Laboratory of Edinburgh, in relation to the question on the existence of combined water in muriatic acid gas. I had found that the salt formed by the combination of this gas with ammonia, affords water when it is exposed to heat; and this water, I inferred, is derived from the acid. Sir H. Davy supposed [illegible] to be water which the salt had absorbed from the air; and he and his brother affirmed, that when the air is excluded, none is obtained. I resumed the investigation, and found that the salt absorbs no water from the air, and that it affords water when heated, though the air has been excluded. The same results were obtained by Drs. Bostock and Traill. It remained, therefore, for Sir Humphry either to shew that they were not correct, or to establish, by farther evidence, his former statement. With this view the experiment, above alluded to, has been performed. About 90 cubic inches of muriatic acid gas were combined with the requisite quantity of ammoniacal gas, in an exhausted retort of the capacity of 26 cubic inches, and the salt formed having been heated in the same retort, closed at its extremity by a stop-cock, water was obtained from it in small quantity, "a dew just perceptible lining the cold neck." On this experiment I have now to offer a few observations, and I have to state the result of another since performed.

Muriate of ammonia does not absorb water from the air.

When the experiment was made, I was informed by Dr. Hope of the result, and of the manner in which it had been executed. I stated to him in what respect it appeared to me objectionable, independent of the unfavourable circumstances inseparable from the mode of heating the salt in a close vessel; the large size of the retort rendering it difficult to apply the heat equally, so as to expel the water from one part without its condensing in another, allowing, too, a larger portion of any vapour disengaged to remain in the elastic form while the heat was kept high, and equally permitting its condensation when the heat diminished over an extensive surface, encrusted with a substance by which it would be absorbed, the unequal application of the heat producing a similar volatilization from one part, and condensation in another, the confinement of the heated elastic fluid operating by its pressure in resisting the separation of the water from the salt, and by its temperature counteracting the local condensation of the portion evaporated, and lastly, the encrustation of salt which had been allowed to remain at the curvature and upper part of the neck of the retort, where, in such an experiment, the condensation of moisture chiefly takes place, were all unfavourable to the result. If the experiment had been one in which a considerable quantity of water was to be looked for, these circumstances might have been of less importance. But this not being the case, it was more necessary to attend to their influence, and every arrange-

The retort in Sir H. Davy's experiment was too large.

And the confinement prevented the separation and condensation of the water.

ment with regard to the experiment, ought to have been rendered favourable to the result, instead of being truly the reverse.

Fit conditions: a smaller vessel, heat equally diffused, no sublimed salt at place of condens. nor pressure, nor vacuum.

The principal circumstances which I conceived required to be attended to, were, to employ a much smaller vessel, to raise it through its whole capacity to an equal heat, to have the part of the apparatus in which the water is to be condensed free from salt, and to avoid, as far as practicable, the operation, either of pressure, or of a partial vacuum. It was nearly in this manner, that the experiment was performed by Dr. Bostock and Dr. Traill, and hence their successful result, while Sir Humphry, from not attending to these circumstances, was less successful, though performing it on a much larger scale. Dr. Hope, anxious to ascertain the matter of fact, readily agreed to repeat the experiment with these variations; Lord Webb Seymour and Mr. Ellis were present, and I have his permission to communicate the result.

Repetition of exp. before eminent men.

Mur. of am. from dried gases was exposed to equal heat in a small retort.

Ammoniacal gas, previously exposed for two days to dry potash, and muriatic acid gas which had been exposed to dry muriate of lime for 24 hours, were combined in a dry exhausted flask, of the capacity of 3·8 cubic inches. About 90 cubic inches of the acid gas were employed, and the flask remained at the end filled with ammoniacal gas: The stop-cock being removed without exposing the salt to the air, a glass tube of four-tenths of an inch in diameter, previously fitted by grinding to the neck of the flask, was inserted, its open extremity dipping in quicksilver, and the flask being surrounded with sand in an iron box, was placed horizontally on a chafing dish, and fuel gradually introduced, so that the heat applied was slowly raised. In a short time moisture appeared in the tube, at a little distance from its insertion into the flask; this increased, proceeding to a greater extent along the tube, and condensing in globules perfectly distinct, which, at different periods of the experiment, covered the inner surface for a length of three, four, or six inches; and a small quantity collected at the under part, which, with a very slight inclination of the tube, moved slowly onward. At length the salt sublimed, and condensed in the tube close to the flask. The quantity of water, Dr. Hope was satisfied, appeared considerably larger than in Sir Humphry's experiment. The same quantities

Water was obtained, viz. about three-fourths of a grain;

tities of gases had been employed as in that experiment, and I need scarcely say, that every precaution had been taken to exclude every source of fallacy. Some of the salt having reached near to that place of the tube where the dew was condensed, part of the moisture seemed to have been resumed by it during the cooling of the apparatus, and prevented Dr. Hope from ascertaining with precision the quantity of the fluid. To obtain an estimate of it, he next day put a little water into another flask having a similar tube, previously weighed, fitted to it by grinding, and applied heat to the flask till the inside of a portion of the tube was covered with dew, and a drop of water collected in the bottom, as in the preceding experiment. The quantity of humidity, thus condensed, weighed one grain, and in appearance so far exceeded that observed in the tube in the experiment of the preceding day, as to lead to the conclusion, that the latter could not be estimated at more than two-thirds of a grain.

which exceeds that obtained by Sir H. D. and establishes the author's statements, &c.

Such is the result of these experiments intended to be decisive of the question with regard to the state of the fact, whether, when this salt is heated in close vessels, any water is obtained from it or not. Messrs. Davys affirmed, in the most explicit terms, that there is none; Sir H. Davy " did not observe the slightest traces of moisture in making the experiment on a larger scale in exhausted vessels." And Mr. J. Davy found, that " no water was produced—not even the slightest trace appeared." I affirmed, that though this mode of conducting the experiment is unfavourable to the result, and is not at all calculated to afford information with regard to the real quantity which the salt yields, still a sensible portion of water is obtained. It is now established, that my statement is correct, that of my opponents the reverse. In the experiment, as performed by Sir Humphry himself, a sensible portion of water appeared, and when the obvious sources of fallacy attending that experiment have been avoided, a larger quantity has been obtained.

Remarks. The vapour in Sir H. D.'s experiment did not come from extraneous sources.

To obviate the conclusion which might be drawn from this result, Mr. J. Davy endeavours to show, that the quantity obtained in his brother's experiment might be derived from extraneous sources, from vapour in the gases, or moisture from the mercury. This it is scarcely necessary to discuss. Dr. Henry, he remarks, found that ammonia obstinately retains aqueous vapour, yet Dr. Henry states, that ammonia may be so far desiccated by exposure to potash, " as to shew no traces of condensed moisture when exposed to a cold of 0° of Fahrenheit," and this precaution of exposing the ammonia to heat had been observed both in Sir Humphry's and in Dr. Hope's experiment. His brother, he adds, has proved, that a minute portion of solution of muriatic acid in water may be obtained by intensely cooling the gas. Dr. Henry, however, found, that muriatic acid gas, when freed from visible moisture, which it is completely by exposure to muriate of lime, (a precaution observed in the above experiments) deposits no water even when cooled to 26 below 0° of Fahrenheit, and Gay Lussac not only obtained the same result, but farther found no indication of moisture from the action of fluo-boric gas, which is its most delicate test. And, even according to Sir Humphry's statement, the quantity of liquid deposited from 200 cubical inches at 75°, cooled to 10 below 0, is not equal to $\frac{1}{16}$ of a grain, and only about half the weight of this is water. If any such water, therefore, is taken up by the gas at 50°, and retained by it after exposure to muriate of lime, of which there is no proof, but the reverse, it may amount, in 90 cubic inches, to $\frac{1}{75}$ or $\frac{1}{80}$ of a grain. Lastly, the mercury had been strained through warm linen, and was perfectly dry. The gases, therefore, having been submitted carefully to processes which are known to render them free from all moisture, being transmitted through dry mercury, and combined in an exhausted vessel, so that the mercury never came into contact with the salt, there is not the slightest reason to suppose a communication of water from any extraneous source. It is an obvious reflection, too, that if this salt is otherwise entirely free from water, as the new hypothesis assumes, were a minute portion communicated to it, it must be retained, in conformity to the law which universally regulates the combination of water with saline substances, by a very powerful attraction, so that it could not be expelled, and rendered sensible in such an experiment. And lastly, such causes are assigned by Mr. J. Davy only as " tending to account for the very minute quantity of water obtained" in his brother's experiment. They are, of course, still less adequate to account for the larger quantity in Dr. Hope's experiment; and are utterly incapable of accounting for the much larger quantity admitted by them to be obtained when the salt is heated in com-

If the salt were greedy of water, it would retain it, &c.

communication with the atmosphere, and which, it will be shewn, is derived from the salt, and not from the air.

Mr. J. Davy farther contrasts the small quantity of water obtained from the muriate of ammonia in his brother's experiment with the quantity which, according to the common doctrine, it contains ; this latter quantity, he seems to imagine, ought to be procured ; and, since it is not, he concludes that that doctrine cannot be maintained.

This kind of exp. is not calculated to show the quantity.

Any discussion with regard to the *quantity* of water obtained by heating the salt in a close vessel, is probably superfluous. That kind of experiment I never considered as one calculated to afford a proper indication of the real quantity which the salt yields. I repeated it merely because Messrs. Davys affirmed, that there is no appearance of water whatever. That assertion is now proved to be incorrect, which is all that the repetition of the experiment was designed to establish, and the original mode of conducting it I consider as the one which gives the true result.

Elucidation from various facts.

It may be remarked, however, to obviate any difficulty from this point, even with regard to the quantity obtained in the more favourable mode of conducting the experiment, that the combination of muriatic acid gas with ammonia, was not regarded as adapted to determine the proportion of combined water in the acid gas ; for, of all the combinations of this acid, it is the one in which there is the greatest difficulty in separating the water. Acids, in combining with salifiable bases, retain the whole, or the greater part of their combined water, especially when these bases have also an attraction to water. To expel this from the compound salt to any extent, a heat, equal or superior to ignition, is in general required ; and, by the most intense heat, it does not appear, that the whole quantity is expelled. Berthollet has shown, that after exposure to the violent heat of a forge, salts retain water, so that when again exposed to heat in mixture with iron filings, they afford hydrogen gas ; and this is the case even with those which appear to have little attraction to water, as sulphate of barytes. Where the salt, therefore, is volatile, such as muriate of ammonia, the expulsion of its water must be imperfectly attained. The degree to which the heat may be raised is not great, and, in raising it, it must operate nearly with as much force on the real salt, as on the water combined with it, and their mutual affinity must retain them in union till both are sublimed together. If other salts which are fixed, and which have a less strong attraction to water, yield it only at a high temperature, and then imperfectly, it is absurd to imagine, that muriate of ammonia should yield it at a much lower temperature, and yield it entirely. The experiment, therefore, was designed rather to prove the *existence* of combined water in muriatic acid gas, and though the quantity obtained may not be the whole quantity which, from other facts, there is reason to conclude, exists in the acid gas, it establishes this as much as if a larger quantity were obtained.

It proves the existence of water, and establishes the doctrine.

The production of any water is incompatible with Sir Humphry's hypothesis, and, therefore, refutes it; it is conformable to the opposite doctrine, and becomes, therefore, a proof of its truth; and for the quantity being less than that from other saline combinations of the acid, an adequate cause can be assigned. The actual result, indeed, is precisely that which is to be expected, a sensible portion of water more considerable as the experiment is performed in a manner more favourable to its disengagement, but inferior to what is obtained from other combinations of the acid, from which it is obvious, *a priori*, that the water must be more easily expelled.

So far I have restricted my observations chiefly to the result of the experiment of heating the salt in close vessels. A point not less important, which remained for determination, is that relating to the result when it is heated in *open* vessels, and to the supposed fallacy connected with this in the absorption of water from the air.

Whether the salt absorbs water from the air, as *supposed* by Sir H. Davy.

I had found that, in this mode of conducting the experiment, a very sensible quantity of water was obtained; and this was not denied, but explicitly admitted, by my opponents. Mr. J. Davy, who had heated the salt in close vessels, without obtaining water, found, that when he "followed Mr. Murray's example, and collected the salt in the atmosphere, and introduced it into another retort, on heat being applied, water, in no inconsiderable quantity, was evolved, as he described." But to account for this, without admitting the conclusion subversive of his hypothesis, Sir Humphry Davy advanced the supposition, that the salt absorbs water from the air during its transference from the one vessel to the other, and that this is the source of the water which it yields.

This supposition was not warranted by the facts;

A supposition so directly at variance with the known properties of this salt, required very ample proof, yet none was given of it, farther than the assertion of the salt not yielding water when heated in a close vessel, while it affords it when heated in an open vessel, this result being stated as affording "a demonstration, that the water liberated in Mr. Murray's experiment, was not derived from the muriatic gas, but from the atmosphere." It affords, I remarked, (Journal, vol. 32, p. 187,) no proof, since, admitting even the statement with regard to it to be correct, it might equally arise, since it is proved, that the salt yields water when it is heated without having been exposed to the air.

and might have been ascertained by direct experiment, but was not.

I had proposed the obvious experiment by which the fact, with regard to this supposed absorption of water, may be unequivocally ascertained—that of forming the salt without exposure to the air, and then ascertaining if, under such exposure, it gains weight, which it must do if it absorbs water. The mode of conducting the experiment, and the results, have been already minutely detailed (Journal, vol. XXXII, p. 191.) These results, proving that no water is absorbed, Messrs. Davys have not attempted to controvert, but have rather thought proper to avoid repeating the experiment, though it had been urged against them, and is obviously decisive of the question—for what reason I shall not conjecture.

Repetition of the experiment with Dr. Hope: Lord Webb Seymour and Mr. Ellis being present. A vessel with wide apertures at each end was taken;

The importance of the fact with regard to this supposed absorption is such, both from the supposition having been introduced to account for the production of water from the salt, and from its having led, in consequence of that, to a form of experiment which has rendered the investigation more difficult and more liable to error, that I was desirous the experiment should again be performed with every precaution. Lord Webb Seymour and Mr. Ellis were present, and the principal steps of the experiment were executed by Dr. Hope. A vessel was selected, the interior of which might admit of a free exposure to the air—it was pear-shaped, having a wide orifice at each extremity, the one, one inch and a half in diameter, the other, one inch, its whole internal surface being equal to about 40 square inches. The orifices were closed with corks rendered air-tight air-tight by cement, a stop-cock being inserted in one of them for the introduction of the gases.

in which the dried mur. ac. and ammon. gases were combined. Careful examination of the salt by the balance, during full exposure to the air, showed no increase, but a loss of weight.

The vessel having been exhausted, about 27 cubic inches of muriatic acid gas, which had been exposed for two days to dry muriate of lime, were combined in it with the requisite quantity of ammoniacal gas, which had been exposed for the same time to dry potash; and an excess of ammonia was allowed to remain at the end of the combination. The corks, with their cement, were removed, and clean corks, previously fitted, were instantly inserted. The vessel was filled with atmospheric air, by opening one of the orifices, and introducing a tube attached to a caoutchouc bottle, the sides of which being pressed together, and then allowed to dilate, drew out the ammoniacal gas: and to secure the change being complete, both corks were removed for a second or two. The apparatus was then placed in a balance, which, loaded with it, turned very sensibly with much less than $\frac{1}{20}$ of a grain. The balance being accurately adjusted, the corks were removed from the orifices, and placed beneath the vessel, and the progress of the experiment was observed. At the end of five minutes there was no perceptible change, of ten minutes no change, at fifteen minutes there was, if any thing, a loss of weight on the side of the salt, at twenty minutes this loss was apparent, and amounted to about $\frac{1}{20}$ of a grain, at twenty-five and at thirty minutes it remained the same. Though from the form of the vessel, and the size of the apertures, the air had the freest access to the salt which encrusted the interior; yet, to leave no doubt, the internal air was changed repeatedly by means of the caoutchouc bottle. At forty minutes there was again the appearance of loss of weight in the salt, at fifty minutes this amounted to something less than $\frac{1}{80}$ of a grain, in addition to the former loss. The air within the vessel was again repeatedly changed, both by means of the caoutchouc bottle, and by propelling the external air through it by the motion of the hand, and by the bottle, held at a distance and slowly compressed; but for half an hour longer there was no perceptible variation of weight*.

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* In a preliminary experiment which I had performed, and in which the salt was freely exposed to the air for three days, the loss of weight was

This experiment was performed in the same apartment in which my former experiments had been executed, and the air was at the same temperature of 60°. It is perfectly decisive in proving, that the salt absorbs no water from air in a common state of dryness and temperature.

A portion of this muriate of ammonia, being exposed to heat, gave out water.

As much of the salt was collected as could be removed from the vessel; it weighed 23·5 grains. It was introduced into a small retort connected with a small globular receiver, and the body of the retort being in part surrounded with sand, heat was applied by a lamp. A little of the salt suddenly rose in vapour into the neck of the retort. Afterwards moisture condensed beyond the salt where the neck was kept cool; the heat was slowly raised until the salt was sublimed into the top and beginning of the neck of the retort. The sand bath was then removed, a chaffing dish was applied, and the heat continued for half an hour. In the course of the experiment, the moisture increased, and extended over about one inch and a half of the upper side of the neck of the retort, where the cold was applied. The half of this space next to the bulb appeared quite wet, being covered with compressed globules of water of a considerable size, on the remaining part the globules were very minute.

More water inferred to have been separated in an exp. with higher heat.

I formerly related an experiment in which muriate of ammonia, after it had afforded a portion of water at a low heat, was sublimed through ignited charcoal, to ascertain if, by the higher temperature, and by the chemical affinities exerted by charcoal, an additional quantity might be abstracted. Portions of carbonic acid, and carburetted hydrogen gases, were accordingly obtained; and a quantity of water was condensed. This latter result led to the conclusion, that the high degree of heat had produced a more perfect separation of the water, and that, therefore, if such a temperature were applied to the salt alone, more water might be obtained from it than by an inferior heat, while any supposed source of fallacy from the presence of the charcoal, might be avoided.

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was apparent to a still greater extent than in the above experiment. Such a result, with regard to any other salt, would be ascribed to the abstraction of water by the agency of the air; and I see no reason why the same conclusion should not be drawn with regard to it. At the end of a week the salt remained perfectly dry.

A fact

Common sal-ammoniac has its first portion of water expelled in the manufactory, and does not afterwards attract more;

but it gives out another portion in an ignited tube.

Exp. of sublimation with the salt of direct combination.

A fact I had ascertained promised to afford a satisfactory mode of verifying this. The common sublimed muriate of ammonia, or sal-ammoniac, I had found, yields no water when exposed to a heat sufficient to sublime it. This is owing to its mode of preparation—it is first dried, then sublimed, and, during the sublimation, the upper part of the vessel is kept hot, to render the sublimed mass sufficiently dense, its orifice being also kept open, and hence all the water which can be driven off by this heat is expelled, and none is regained by exposure to the air (a decisive proof, if such were wanting, that this salt attracts no water from the atmosphere, since it is kept in the shops without any particular precaution. I exposed 100 grains of this salt in a retort to a heat sufficient to produce sublimation, but no moisture appeared during any part of the experiment. I then sublimed 100 grains of the same salt from the close end of a porcelain tube, placed across a furnace so as to be at a red heat. A very sensible quantity of moisture condensed in a glass tube, which was adapted to the porcelain one, appearing not only in globules, but at length running down the tube. This proved, that water may be separated from muriate of ammonia by a red heat, which is not expelled from it at a lower temperature. I then submitted to a similar experiment, the salt formed by the direct combination of its elements. Very little moisture appeared previous to its actual volatilization, but when this commenced, the condensation of water in sensible globules took place; they continued to accumulate, and the quantity appeared obviously greater than what, judging from former experiments, would have been obtained by a lower heat from the salt formed from the same quantity of muriatic acid gas.

Repetition in an ignited tube.

In another experiment, the salt formed in an exhausted retort was first heated until it ceased to afford water, and was afterwards sublimed through an ignited porcelain tube. Moisture was again obtained, though not in so large a quantity as when the charcoal had been placed in the tube. There is no just objection to the introduction of the agency of the charcoal, if care be taken to have it thoroughly calcined; and, as the supposed source of fallacy from the air affording water to the salt, is now proved to have no existence, there is no valid objection to the result which the experiment with the charcoal affords.

My preceding conclusions, I trust, are now sufficiently established, and it is unnecessary to enter on any recapitulation of the argument. Water has been obtained from this salt both when it is heated in close and in open vessels; and no source of fallacy exists, as was affirmed by Messrs. Davys, in any absorption of water from the atmosphere. They accounted for the production of water on that supposition, and it is now amply refuted.

Inference. The salt does afford water, and did not acquire it from the atmos.

I have only a single observation to make on Mr. J. Davy's concluding remarks in his last communication, that he has "no intention of answering personal aspersions, which are only injurious to the author when unjustly made." The necessity was imposed upon me by assertions which he had advanced of stating some circumstances connected with the manner in which he and his brother had conducted the controversy. I did so with reluctance, and only in so far as was necessary to my own vindication from a very intemperate attack. My observations conveyed censure, no doubt, but not aspersion; for they were founded on facts, and these were very explicitly stated, that Mr. J. Davy might, if he pleased, enter into any explanation with regard to them. This he has not done, and the facts, I believe, he is unable to controvert.

Considerations relating to personal aspersion and censure.

In concluding this investigation, I cannot but contrast the assertions that were made, and the tone that was assumed, with the result that has been established. "At first view," said Mr. J. Davy, speaking of my experiment of obtaining water from muriate of ammonia, "the result appears improbable, and opposed by several facts; and, in a very short time, I was convinced by experiments that it was incorrect." Again, "Mr. Davy, my brother, informed me, that he had not observed the slightest traces of moisture in making the experiment on a large scale in exhausted vessels; and assured me, that I should not, was not the salt exposed to the atmosphere." In repeating the experiment accordingly, no water was produced "agreeably to my brother's result, not even the slightest traces appeared." Mr. Murray's errour," he adds, "appears to have arisen partly from too great confidence placed in the accuracy of his experiment, and partly from overlooking, that

From a review of the manner in which the author's results were discussed, he suggests, that it might, with propriety, have been more modest and temperate.

that a light powdery substance, like muriate of ammonia, independent of its chemical attraction, absorbs water hygrometrically. Mr. Davy has informed me, that this is the case, and that muriate of ammonia so made, absorbs so much, that it even deliquesces." And lastly, "Mr. Murray's confidence in his result, which is opposed by several facts relative to muriate of ammonia, is to me more surprising than the result itself." When assertions and conclusions have been advanced in this unqualified manner, which the result of investigation proves to be wholly incorrect, it is but justice to recall them for a moment to notice; and when such a style of controversy has been indulged in, it is not uncandid to suggest the reflection, how much more becoming would have been a more modest and temperate tone. I shall refrain from farther animadversion on a topic ungrateful in itself, and too unimportant to claim any protracted discussion.

With the highest respecf,
I remain
Your most obedient Servant,
J. MURRAY.

V.

On the Explosive Compound of Chlorine and Azote.

(Concluded from p. 190.)

To Mr. Nicholson.

SIR,

IN conformity with our promise made to you in our former communication, we resume the account of our experiments with the explosive compound.

Globule of the compound exposed under water to voltaism.

A globule of the compound was placed under water, between the ends of two platina wires, coated with glass excepting the points; one of these wires communicated with the positive, and the other with the negative end of a voltaic trough, containing 50 pairs of six-inch plates, excited by weak muriatic acid. The globule appeared to be little, if at all, affected by the current of the electric fluid, of which, we are inclined to believe, it is not a conductor: small bubbles of gas rose from it occa-

No effect under these circumstances.

occasionally, but as the water with which it was in contact was undergoing rapid decomposition, it is not unlikely that these bubbles were not caused directly by the electric fluid, but by the hydrogen or oxigen liberated from the water, acting on the compound. This source of ambiguity would be removed, if the compound could be electrised without its being in contact with any fluid; but its extreme volatility presents an obstacle to such an arrangement, which we have not yet surmounted.

Having made a number of experiments for the purpose of ascertaining generally the phenomena resulting from the contact of various substances with the explosive compound, we have made out the following table, in the first column of which are stated the several substances employed, and in the second the apparent effects of them on the compound. It is proper to remark, that water was always present in these experiments, the general method of making them having been to place a globule of the compound in a small iron ladle filled with water, and to bring the substance, whose action on it was to be tried, into contact with it at the bottom; but in those cases in which it was desirable to have as little water present as possible, we have substituted for the ladle a little paper filter, containing the compound and water, and allowed nearly all the water to drop through, before we added the substance to the compound. This was our mode of operating with ether, alcohol, &c.

Phenomena produced by contact of the explosive compound, and various substances.

Table of the apparent Effects of certain Substances brought into Contact with the explosive Compound.

Substances brought into contact with the compound.	Effects observed.
Mercury	Slight effervescence, the metal slowly tarnished.
Copper	do. do.
Tin	None.
Zinc	None.
Sulphur	None.
Liquid sulphuretted hydrogen, or alcohol of sulphur	None.
Super Sulphuretted hydrogen formed by adding hydroguretted sulphuret of potash to muriatic acid. ..	Violent explosion.

Effects of substances brought into contact with the explosive compound.

Do.

Effects of substances brought into contact with the explosive compound.

Substances brought into contact with the compound.	Effects observed.
Do. become solid by keeping.	Union, but no explosion
Native sulphuret of antim.	None.
Red sulphuret of mercury.	None.
Phosphorus	Explosion extremely violent.
Phosphuret of lime	Violent explosion.
Phosphorus dissolved in liquid sulphuretted hyd...	Do.
Charcoal	None.
Jet	None.
Cannel coal	Adhesion, slight effervescence.
Residuum of the distillation of amber	Effervescence, film on the surface of the water.
Asphaltum	Union, rapid effervescence, ascent to the surface of the water, film left there.
Elastic bitumen	Union.
Elastic gum, or caoutchouc..	Violent explosion.
Resinous matter found in Highgate Hill	None.
Common resin	Effervescence.
Shell Lac	None.
Copal	Union, effervescence, ascent to the surface of the water, film left there.
Sandarach	Adhesion.
Mastich	Same as with copal.
Euphorbium	Do. do.
Guiacum	Do. do.
Assafœtida	Do. do.
Opium	Slight effervescence, film on the water.
Burgundy pitch	Union, effervescence.
Balsam of Tolu	Do. do. rapid, film on the water.
Resin of ox bile	Slight effervescence.
Myrrh	Explosion.
Scammony	None.
Frankincense	Do.
Ammoniacum	Do.
Hepatic aloes	Do.
Alcoholic solution of resin ..	Rapid effervescence.
Do. of resin of lac	Do.

Cam-

Substances brought into contact with the compound.	Effects observed.	Effects of substances brought into contact with the explosive compound.
Camphor	Union in considerable quantity with the compound, which preserves its usual colour and appearance. When the camphoretted compound rises to the surface of the water, it covers it with a film of camphor, the explosive compound escaping from it. The camphoretted compound inflames without explosion by phosphorus and by essential oils. Sulphuric ether separates the camphor from it. It may be formed at the same time with the explosive compound, by introducing a bit of camphor into chlorine gas over solution of muriate of ammonia.	
Phosphurretted camphor ..	Explosion.	
Sulphuretted do.	Union.	
Wax	None.	
Spermaceti	None.	
Adipocire	None.	
Butter	None.	
Palm oil	Explosion.	
Do. saturated with chlorine, which made it white and semifluid	Union.	
Oil of mace	Do. rapid effervescence, film on the water.	
Ambergris	Explosion.	
Hogs' lard	None.	
Whale oil	Explosion, separation of carbon.	
Linseed oil	Explosion.	
Olive oil	Do. separation of carbon.	
Do. do. saturated with chlorine	Union.	
Do. do camphoretted ..	Violent explosion.	
Do. do. sulphuretted	Do.	
Do. do. thickened by boiling on oxide of mercury.	Effervescence, explosion.	

Do.

ffects of sub- ances rought into ontact with e explosive mpound.

Substances brought into contact with the compound.	Effects observed.
Olive oil, by boiling on corrosive sublimate	Union.
Oil from soap by sulphuric acid	Union, effervescence, film on water.
Do. do. by nitric acid	Do. brisk do. film on water.
Oil of turpentine	Violent explosion.
Do. do. sat. with chlorine	Union; by the application of flame to it on the surface of water, it deflagrates, and leaves a resinous looking substance on the water.
Oil of tar.	Violent explosion.
Do. of amber	Do.
Do. of petroleum	Do.
Do. of Benzoin	Union, effervescence, remarkable change of colour to blood red.
Do. of orange peel	Violent explosion.
Naptha	Rapid effervescence, explosion.
Alcohol	None.
Sulphuric ether	None.
Nitric ether	None.
Phosphuretted ether	Violent effervescence.
Soap of potash	Rapid effervescence.
Do. of soda (curd soap)	Union, effervescence, film or water.
Do. of do. (Castille)	Do. Do. Do.
Do. of barytes (from nitrate)	Slow effervesence.
Do. of alumine (from sulphate)	Much effervescence.
Do of lime (from nitrate)	Slow effervescence.
Do. of strontia (from do.)	Do. Do.
Do. of magnesia (from sulphate	Do. Do.
Do. of silver (from nitrate)	Violent explosion, blue flame.
Do. of protoxide of mercury (from nitrate.)	Do. Do. white flame.
Do. of peroxide of mercury (from nitrate.)	Do. Do.
Do. of copper (from nitrate.)	Do. Do.
Do of lead (from nitrate.)	Do. Do.
Do. of do. (litharge plaister.)	Do. Do.
Do. of tin (from muriate.)	Effervescence.
Do. of cobalt (from nitro-muriate.)	Do. Do.

Substances brought into contact with the compound.	Effects observed.	Effects of substances brought into contact with the explosive compound.
Soap of platina (from nitro-muriate.)	Effervescence rapid, film on water.	
Do. of manganese (from sulphate.)	Violent explosion.	
Sugar	None.	
Manna	None.	
Gum Senegal	None.	
Starch	None.	
Indigo	None.	
Kino	None.	
Catechu or terra japonica.	None.	
Extract of logwood.	None.	
Benzoic acid.	None.	
Albumen (dried.)	None.	
Prussiate of iron.	None.	
Triple Prussiate of potash in crystals.	None.	
Sulphuric acid.	None.	
Nitric acid	None.	
Muriatic acid	None.	
Phosphorous acid.	None.	
Fused potash pure.	Explosion, owing to the heat produced by the potash dissolving in a small quantity of water.	
Solution of do.	Effervescence.	
Solution of pure ammonia.	Violent explosion.	
Do. diluted with its bulk of water.	Rapid effervescence.	
Lime	Effervescence.	
Carbonate of lime.	Do.	
Red oxide of lead.	Do.	
Nitrate of silver.	Muriate of silver formed, the compound disappeared.	
Hidrogen gas.	The compound disappeared immediately, the volume of the gas increased.	
Super-carburetted hidrogen, or olefiant gas.	The compound disappeared immediately.	
Phosphuretted hidrogen gas.	Do. with explosion.	
Sulphuretted do. do.	Do. opacity in the gas, precipitation of sulphur.	
Arseniuretted do. do.	Do. precipitation of arsenic.	
Oxigen gas.	Do.	

Effects of substances brought into contact with the explosive compound.

Substances brought into contact with the explosive compound.	Effects observed.
Azotic gas.	Do. precipitation of arsenic.
Atmospheric air.	Do.
Nitrous gas.	Do. violent explosion, blue flame.

Remark upon the table, and the general effects.

In performing the experiments, the results of which are stated in the preceding table, our intention was not to investigate minutely the changes produced in the substances made to act on each other, but to acquire a knowledge of the principal and most obvious effects of the explosive compound on a variety of bodies. This, we trust that we have accomplished; and, in so doing, have discovered some curious and interesting facts; amongst which the following appear to be most deserving of notice.

Combustibles act most strongly on the compound.

1st. The class of bodies which act on the explosive compound with the most energy, are those which are termed combustible bodies. There are, however, some few exceptions to this remark, instanced in the want of action of ether and of alcohol.

The effects appear to arise from dense chlorine.

2d. That there is a considerable analogy between the action of the explosive compound, and that of the chlorine and euchlorine, separated in a condensed state by strong sulphuric acid, from the salt, known by the name of the oxymuriate of potash; which, like the explosive compound, inflames volatile oils, caoutchouc, phosphorus ammonia, &c. And that most of the effects of the explosive compound are attributable to chlorine in a condensed state, and in weak chemical union.

Camphor, &c. unite without decomposition.

3d. That there are some combustible bodies which will unite without decomposition, with the explosive compound, of which camphor is a remarkable instance.

No action with saturated bodies.

4th. That when a combustible body is previously saturated with chlorine, its action on the explosive compound is either annihilated or much weakened.

Animal substances have less action than vegetable.

5th. That animal substances in general appear to act with less energy on the explosive compound, than their analogous vegetable substances. The want of action of adepocire, of spermaceti, of butter, and of lard, are striking proofs of the truth of this assertion.

Soaps, by double de-

6th. That there is a remarkable difference in the actions on the compound of the several soaps formed by double decomposition

composition, do not explode.

position of saline solutions, and solution of soap; as it appears that the earthy soaps do not explode with it, and that of the metallic soaps, those prepared from nitric salts explode, while those prepared from muriatic salts do not.

The experiments may, in some cases, vary on repetition.

Of the numerous experiments, of which a statement is given in the preceding table, we will not pledge ourselves that all are equally accurate; we have taken considerable pains that they should be so, but their number has hitherto prevented us from repeating the greater part of them. The repetition of some of them has convinced us, that very minute circumstances will sometimes cause the results to vary. Should we hereafter find it necessary to correct any involuntary inaccuracies in our statement, we shall do it with confidence in the indulgence of the readers of your Journal.

Apparatus for exposing the compound to gas in a closed vessel.

As it may be expected, that we should describe our mode of bringing the explosive compound into contact with confined portions of the gases, we have represented our apparatus for this purpose in the sketch, plate VI. fig. 3, to which the present explanation will apply. (*a*) A small capsule of bone or ivory, having a small hole in its centre—this capsule is suspended by a string, passing air-tight through the top of a glass receiver (*e*) between a collar of wetted leather, which serves to secure in its place the stop-cock (*b*)—this stop-cock has a connecting screw, to which the stop-cock (*c*) of the bladder (*d*) can at any time be attached. (*f*) is a water bath.

and method of using it.

When this apparatus is to be used, the capsule (*a*) is to be drawn down, so as to bring it on the outside of the glass receiver: the bladder with its stop-cock is to be unscrewed and filled with the gas intended to be used; the stop-cock (*b*) is to be opened, and by the action of the mouth applied to it the water is to be drawn up so as to fill the receiver. The cock (*b*) is then to be shut, and the cock (*c*) with the bladder of gas screwed on. A small piece of blotting paper is then to be laid on the hole in the capsule, on which the globule of the explosive compound is to be placed. The capsule is then to be placed again under the receiver, and by means of the string on the outside, drawn up into the receiver full of water, to such a height as may be thought necessary; after which, the two stop-cocks are to be opened, which will admit the air from the bladder into the receiver; the water in which will all descend as the air enters, excepting what is retained in the capsule, and which covers the globule of the compound; but as this small quantity very quickly filters through the blotting paper, and falls in drops through the hole in the capsule, the compound is left exposed to the gas, and the effects of this exposure immediately appear.

When the compound is kept with water in a sealed tube, it becomes dissolved in process of time, unless the quantity of water be inconsiderable.

In our former communication we mentioned that the compound may be preserved for any length of time, in small tubes hermetically sealed, provided that the quantity of water, or of air, included with it, did not exceed seventeen times its bulk; we have since found that this is strictly true, only when the quantity of water in the tube is very inconsiderable compared to that of the air included; for that when the tube is nearly filled with water, the compound, after some months, disappears and is dissolved in it.

Remarks on sources of errour in the former analysis.

In the same communication we described an analysis of the compound, remarking, at the same time, that not having repeated it, we could not place any confidence in its results, and that our principal object in giving an account of it was, to show an easy and practicable mode of analysing the compound. In the interval, since that was published, we have paid particular attention to that analysis, and have found that there were two very material sources of error in it; the first was owing to the imperfect means which we then possessed of obtaining two globules of equal weights; and the second, to a circumstance of which we were not then aware (but which our subsequent experiments have proved to have a considerable influence) viz. that the quantity of water with which the explosive compound was in contact when it was decomposed by potash, was much too large, and occasioned less azotic gas to be given out, than would otherwise have been collected. To obviate these two sources of error has been the object of our recent labours, and we have fortunately succeeded in removing both.

The globules of the compound were unequal, and too much water was present.

Remedy. The exact measure of the compound was ascertained by a capillary syringe,

The mode by which we have succeeded in always operating with known weights of the compound, is by using a glass syringe, of the form represented in fig. 4. pl. VI. the lower part of which terminates in a tube of small bore; such as is used for thermometers. This tube is graduated on the outside into inches and decimal parts, and when the point is placed in a globule of the explosive compound under water, and the piston raised, the compound may be drawn up in an uninterrupted line to any mark on the stem that may be desired: the inch measure of the stem of our syringe holds exactly 5·3 grains of pure mercury; consequently it must hold ·625 of a grain of the explosive compound, the specific gravity of mercury being 13·568, and that of the explosive compound, according to our experiments, being 1·6.

The mode by which we obviated the errour arising from having much water present, was to decompose the compound over mercury in the following manner:

and the quantity of water taken very small, over mercury.

A small stoppered phial was converted to an air receiver, by having its bottom cut off—it was then sunk up to its neck in the mercury contained in a small mercurial trough, the stopper being first taken out. The capacity of the neck was then filled with a few drops of water, into which was introduced the ·625 of a grain of the explosive compound—the glass stopper was then put into its place, and the receiver, with its contents, raised on to the shelf of the trough. Some potash was then procured, which was free from carbonic acid, and had been deprived of any combustible matter, by having undergone igneous fusion; it was also free from any metallic oxide. Of this potash a concentrated aqueous solution was made, and this solution passed up into the receiver to the compound, the decomposition of which it occasioned. In performing this experiment, it is of importance not to pass up the fused potash in the solid state, as the heat which is occasioned by its solution in the small quantity of water which it meets with, instantly causes the compound to explode.

Solution of potash added,

The decomposition of the compound by liquid ammonia we effected exactly in the same manner, passing up the solution of ammonia instead of the solution of potash; the solution of pure ammonia must, however, be diluted with its own bulk of water, otherwise it will immediately occasion an explosion of the compound.

or of ammonia.

By these arrangements, we believe that we have removed every source of error; and, having repeated the analysis several times with the greatest care, and with scarcely any variation in the quantities of gases obtained, we are enabled to give those quantities with considerable confidence. This will be best done by stating the particulars of two of our experiments.

The analysis thus made is entitled to confidence.

Exp. 1. Decomposition by potash.

1st Exp. (Barometer 30·4, thermometer 55°) ·625 of a grain of the explosive compound was decomposed by solution of potash, in the manner just described; the quantity of gas obtained was ·25 of a cubic inch; phosphorus was sublimed in it; after which operation, and being again cooled, its volume was ·245 of a cubic inch; which being phosphuretted azotic gas, must be corrected for an increase of volume of $\frac{1}{40}$ by phosphorus in solution. This brings it to ·239, which, brought to the mean temperature and pressure, becomes ·2447 of a cubic inch, being the quantity of azotic gas derived from the compound.

Exp. 2. Decomposition by ammonia.

2d Exp. (Barometer 30·4, thermometer 55°) ·625 of a grain of the explosive compound was decomposed by a solution of pure ammonia, diluted with its bulk of water, in the manner before stated. The quantity of gas obtained was ·395 of a cubic inch: after subliming phosphorus in it, it was ·41, which, corrected for increase of volume by phosphorus in solution, for pressure above the mean, and for temperature below it, becomes ·4095 of a cubic inch, being the quantity of azotic gas derived both from the compound, and from the decomposition of the ammonia by the chlorine of the compound. Then to know how much is derived from the latter source only, we have only to deduct the quantity ascertained by the first experiment, from that ascertained by this experiment. This being done, the remainder is ·1648, which remainder, representing three times its volume of chlorine gas, gives ·4944 of a cubic inch, as the volume of chlorine gas contained in ·625 of a grain of the explosive compound.

The quantities of azotic and chlorine gases in the compound

The quantity of azotic gas in ·625 of a grain of the compound being ascertained by the first experiment, and that of the chlorine gas in the same weight of the compound being known by the second experiment, it is obvious, that if the experiments are accurate, and the compound consists of chlorine and azote only, then the weights of those two gases should exactly correspond with the weight of the compound, viz. ·625 of a grain.

But, according to the following calculation, this is not the case.

Weight

c. inches.	
Weight of ·2447 of azotic gas (Biot and Arrago)	·0735 of a grain,
Do. of ·4944 of chlorine gas (Gay Lussac and Thenard)	·3724 of do.
Total	·4459 of do.

Here, then, we have a deficiency of ·1791 of a grain, for which we must account either by concluding that our analysis is inaccurate, or that the explosive compound contains some other constituent part besides azote and chlorine.

are less than the whole weight.

But from having repeated our analysis several times, we are convinced, that it is free from errors of any consequence: we, therefore, conclude, that azote and chlorine are not the sole constituents of the explosive compound.

But as the analysis was accurate,

What other, then, does it contain? To answer this question we must first consider what others it can possibly contain, and we shall find that no others, excepting oxigen or hydrogen, can possibly enter into its composition, because, in the simplest cases of its formation, no other bodies are present than chlorine, azote, oxigen, and hydrogen.

the compound must have some other component,

Now, if it contained oxigen as a third substance, the results of the decomposition of the compound by ammonia, would be different from what we find them; for, in that process, the oxigen must either assume the gaseous form, which it does not; or, if it be supposed to form water with the hydrogen of the ammonia, then it must displace five times more azotic gas than chlorine would, because any given weight of oxigen combines with five times more hydrogen than the same weight of chlorine does. Instead, therefore, of collecting too little azotic gas, we should have had a very considerable excess.

which does not appear to be oxigen,

The supposition, therefore, that oxigen is the third substance contained in the explosive compound, is, in the highest degree, improbable, and inconsistent with the results of our experiments.

It must, therefore, be hydrogen which is the third substance.

but hydrogen.

But in whatever proportion the hydrogen may exist in the compound, it must, by combining with a certain portion of the chlorine in that compound, neutralise the decomposing

Deduction, that the hydrogen, being in combination

action

with chlorine, amounted to a quantity stated.

action of that portion on ammonia. This portion of chlorine would not, therefore, be indicated in the action of the compound on ammonia by the separation of one-third its volume of azotic gas, it having combined with hydrogen at the expence of the compound, and not at the expence of the ammonia. In the before-mentioned analysis there is, therefore, a deficiency of a certain portion of hydrogen, and of a certain portion of chlorine; but, as the total deficiency of both is known, (being the difference between the weight of the compound analysed, ·625 of a grain, and that of the gases ascertained by the analysis, ·4459 of a grain) viz. ·1791 of a grain; and as the proportions in which chlorine combines with hydrogen are also known, (being equal volumes of the two gases, or by weight 1 hydrogen to 30·148 chlorine) it follows, that the volumes and weights to be added are,

c. in.		grain.
·23012	of chlorine gas weighing	·17335
·23012	of hydrogen gas, do.	·00575

Whence the component parts, as corrected, are ascertained;

With these additions to the analysis, the composition of ·625 of a grain of the explosive compound will be as follows:

c. in. at mean temp. and pres.		grains.
·7245	chlorine gas	·54575
·2447	azotic gas	·07350
·2301	hydrogen gas	·00575
1·1993	condensed in the compound $\frac{1}{717}$ their volume	·625

or otherwise by a different arrangement of the components.

Or its composition may be stated in a different form, upon the supposition that the elements arrange themselves in the following way; the hydrogen, with part of the chlorine, being in the state of muriatic acid.

c. in. at mean temp. and pres.		grains.
·4944	chlorine gas	·3724
·4602	muriatic acid gas	·1791
·2447	azotic gas	·0735
1·1993	condensed in the compound $\frac{1}{717}$ their volume	·625

An hypothesis may be made that oxigen and hydrogen may be present in the form of water.

An objection may be made to the above reasoning and conclusion, on the ground that we have not taken into consideration the possibility of the explosive substance being a compound of chlorine, azote, oxigen and hydrogen; and it may be said, that the arguments for the exclusion of oxigen from the compound, drawn from its action on ammonia, will lose all their force if it is considered as a quaternary combination instead of a ternary one; because the oxigen and hydrogen in the compound may be in the state of water; in which case neither of them would appear in the gaseous state by the action of ammonia, nor could the oxigen displace azote from that alkali.

But this quaternary compound is not probable.

In answer to such an objection we have to observe, that the supposition that the explosive substance is a quaternary compound of chlorine, azote, oxigen and hydrogen, being at present unsupported by experiment, we conceive that the following reasons will justify us for refusing to admit it.

Reasons.
1. The rule of philosophising.

1st. It is not consistent with the cautious principles of philosophical reasoning to admit four elements in a compound, so long as its properties and actions on other bodies are explicable by three.

2. That such a compound would have different properties.

2d. From the known affinities of the four elements above-mentioned, and from the proportions in which they must exist in the explosive compound on the supposition under consideration, we infer that they must be combined in ·625 of a grain of the explosive substance, in the following manner:

grs.		
·372	chlorine	forming muriatic acid,
·011	hydrogen	
·773	azote	forming nitrous gas,
·083	oxigen.	
·076	oxigen	forming water.
·010	hydrogen	
·625		

In which case the characteristic properties of the compound would be those of muriatic acid and of nitrous gas, and not those of chlorine, which is contrary to the fact.

But if experiments should establish such

Should, however, it be proved, by satisfactory experiments, that the explosive substance contains oxigen, our statement of

the

a compound, the deduction must be changed.

the composition of that substance must be modified, by admitting, that the hydrogen in the compound is neutralized by oxigen instead of by chlorine.

The hydrogen is the link of union,

The hydrogen in the compound appears to be the link, connecting together the chlorine and azote, by its affinities for both, in the same manner as it does in the ternary compound with chlorine and carbon, formed when supercarburetted hydrogen gas and chlorine gas are mixed together.

and prevents the compound from being decomposed by water.

It is the hydrogen in it also that, in all probability, prevents it from being instantly decomposed by water, by weakening the attraction between the chlorine of the compound and the hydrogen of the water; so that it is not able to overcome that which unites the elements of the water.

Instead of Sir H. Davy's theory of chlorine, the earlier theory of ox. mur. acid may probably be applicable.

It will be observed, that we have adopted the system of Sir H. Davy, with respect to the nature of what was formerly called oximuriatic gas. The several phenomena resulting from the action of the explosive compound on other bodies, however, are probably also capable of explanation on the old theory; and such of your readers as may wish to apply that theory to these phenomena, have only to consider them as resulting from the transfer of oxigen from the oximuriatic acid of the compound to the combustible body, forming oxigenised products, such as carbonic acid, oxides, &c. and to the separation of the muriatic acid from the compound; in consequence of this loss of oxigen.

Explanation.

On Sir H. Davy's system, these phenomena are considered as resulting from the attraction of the chlorine for combustible bodies, and most usually for hydrogen, which it takes either from a combustible body containing it, or from the water present; and, in these cases, the muriatic acid is formed from this union of chlorine with hydrogen; and oxygenised products are also formed whenever the hydrogen, which thus unites to the chlorine, is derived from the water.

We are, Sir,

Your most obedient, humble Servants,

R. PORRETT, Jun.
W. WILSON.
RUPERT KIRK.

London, 16th March, 1813.

VI.

Vindication of the Claims of the American Boy to extraordinary Talents and original Discovery. In a Letter from Mr. W. Saint.

To Mr. Nicholson.

SIR,

Observations and facts in support of the talents, and originality of the methods of computing by Zerah Colburn.

IN reading your last number, I was struck with surprise (in common, it should seem, with most of your readers) to find that you had inserted a letter from the *Morning Chronicle*, which purported to give an account of the manner by which the *American boy* performs his calculations with such wonderderful celerity. Now I am persuaded, Sir, that, had you had sufficient leisure to examine into the merits of that letter, and into the claims of its author to the important discovery which he affects to have made, you would not have given publicity, (and, what is of still greater consequence, *your sanction*) to a statement so little calculated to effect the object of its author, which was " to reduce the child to what he really is—a very clever boy, but no prodigy."

Your insertion of this letter, after the very excellent account you gave of the boy in a former number, has tended to produce a belief in the minds of such of your readers as are unaccustomed to abstruse calculations, that what this child does may likewise be effected by *any other boy of good abilities*, and thus a prejudice may be excited against this youthful and astonishing calculator, which may prove equally injurious to his *own fame*, and to his father's *pecuniary interest*. I have, therefore, to request, Sir, that you will assist me in my efforts to vindicate the reputation of this extraordinary boy, by inserting in your *next* number, if convenient, the following remarks on the letter alluded to, in which I have endeavoured to show, that Mr. A. H. E. has not succeeded in discovering the method by which this boy performs his calculations with such surprising celerity.

In the application of M. *Rallier's* method to the extraction of the *cube* root, Mr. A. allows, that "the result is *ambiguous* where the number proposed terminates with an *even* digit, or with a *5*;" he proceeds, however, to explain how the difficulty may

Observations and facts in support of the talents, and originality of the methods of computing by Zerah Colburn.

may be removed with respect to the *even* digit, though I think I may safely challenge him *to produce a single instance of a child from* SIX to EIGHT *years of age*, who would be able to *comprehend* the method, much less to apply it with *facility* and *rapidity*. Be this as it may, it is confessed by Mr. A. that the case of numbers ending with 5 is one which "*can deceive*," and I accordingly expected to find that Mr. A. had given the boy various examples of this *ambiguous case*, and that he had uniformly found the boy incapable of answering such questions *correctly*, or that he had obtained from him *an acknowledgement* that such questions were beyond the reach of his powers to answer. Yet nothing of this kind is mentioned by Mr. A. who leaves us totally in the dark upon the very point which would have cleared up the difficulty. Are we to imagine, then, that Mr. A. though aware of the importance of putting such questions, for the purpose of ascertaining whether M. *Rallier's* method was employed or not, yet omitted to ask them? Or, if he did ask questions of this kind, and received *wrong* answers, (which must have been the case if the boy employed the *method alluded to*,) how is it that he has neglected to avail himself of the statement of this circumstance, so materially affecting his claims to a discovery which he evidently considers to be an important one.

But allow me, Sir, to examine the merits of this rule in its application to the *square* root. Let us suppose the boy was requested to extract the *square* root of the number 42436; here it is obvious the first figure of the root would be 2, and the last either 4 or 6;—if 4 be taken, then 4 or 9 would be found to be the *middle* figure; but if 6 be used, then 0 or 5 would be the *middle* figure; hence there would be no fewer than four different roots obtained by M. *Rallier's* method, of which four the boy could not possibly know the *correct* one, and he might assign either 206, 256, 244, or 294 for the root of the required number. This is no *particular* example, selected for the purpose of exhibiting M. *Rallier's* rule in the most *unfavourable point of view*; for it will be found upon trial, that had *any other number* been proposed, *four different results would have been obtained by this rule*; and that if a number ending with 5 had been proposed, no less than *ten different results would have been produced*, since all square numbers ending with 5 will likewise terminate with 25, as I have shown in your Philosophical Journal, No. 99, where may also be seen some other curious properties relating to square numbers. It is manifest, therefore, that, if the boy adopted *this method*, he would not only make "many more errors in the extraction of the *square* than in that of the *cube* root;" but that he would, in most cases, fail *three times out of four*; and, in some cases, *nine times out of ten*.

Observations and facts in support of the talents, and originality of the methods of computing by Zerah Colburn.

Any of your readers may satisfy themselves respecting this ambiguity, by referring to a table of square numbers, where they will find that the *first 25 square numbers* contain all the varieties of the *two terminating figures* of such numbers; and that the squares of all numbers *equally above and below* 25; as of 24 and 26; or of 23 and 27, &c. will have their two last figures the same: this property may not have been noticed by your readers in general, but those of them who are but slightly acquainted with mathematics may satisfy themselves of its truth and universality; for since the difference of the squares of the sum, and difference of any two numbers is equal to four times the product of those numbers, it is manifest that the difference of the squares of two numbers of the form $25+a$, and $25-a$, would be of the form $100a$; that is, this difference would be *some exact multiple of* 100; and therefore two such squares could not differ in their *units* and *tens* places of figures; viz. in their *two last digits*; hence, then (since the *two* last figures *only* are used in M. *Rallier's* method) would arise the ambiguity which I have stated. It will be easily seen, that what I have shown of numbers of the form $25 \pm a$, and $25-a$, is equally true of the general formulas $25n \pm a$ and $25n-a$.

Having proved, that M. *Rallier's* rule is only of *partial* utility in the extraction of the *cube* root, and of little or no use in the *square* root, "I think it would be *extremely* unfair to conclude, that either this method, or one very similar to it is adopted by the boy.

Suppose, however, Sir, that it were possible for the boy to have answered such questions as related merely to the *square and cube roots* of numbers by the help of the above rule, still this will not explain the method by which he *multiplies four figures by four, or by which he ascertains the factors of any number, however large, with a rapidity that has astonished some*

Observations and facts in support of the talents, and originality of the methods of computing by Zerah Colburn.

of the first mathematicians in the country. I am aware, indeed, that Mr. A. refers to another memoir of M. *Rallier*, *on prime and composite numbers*, and I regret, in common with most of your readers, that he has not given us so much as a *single hint* respecting the method employed in this *second* memoir, though he says "it is probably the one pursued by the boy to find prime numbers, and to resolve numbers into their factors." Without knowing *myself*, however, what this method may be, I cannot think that it has been adopted by the boy, for several reasons; *first*, because it has been known for nearly *fifty years*, *secondly*, that none of the mathematicians who have seen the boy (except Mr. A.) have considered any of the *known* methods of operating with prime and composite numbers, as sufficient to account for the *rapidity with which the calculations have been performed*; and *thirdly*, that the method itself could never have fallen into *disrepute*, but would have been adopted not only by every *mathematician*, but by every teacher of *arithmetic in the most obscure country villages*, if it had been of such inestimable utility as to have enabled boys of ONLY SIX YEARS OF AGE *to have performed such astonishing calculations*.

Again, Sir, Mr. A. made no *new* discovery when he found that the boy, in extracting the square or cube root of any proposed number, made use only of the *two first* and *two last* figures. This curious and singular fact had been known for many months to several eminent mathematicians who had visited the boy, and who were soon convinced, from the *quickness* and *accuracy* of his answers, and from the power which he possessed of correcting himself whenever he committed an error, that M. *Rallier's* method was not the one he employed, *even in the extraction of roots*, much less in ascertaining the *factors of large numbers*, which he does with a *rapidity and apparent facility*, astonishing to those who have been long acquainted with the method alluded to, and who, notwithstanding their years of practice in abstruse calculations, find, that *they themselves* cannot perform such operations, neither by *that method*, nor by any other yet made public! What, then, shall we think of Mr. A.'s claims to the discovery of the "*modus operandi*?"

Mr. A. might have spared himself the trouble of suggesting an *alteration in the intermediate figures of any perfect cube*,

which

Observations and facts in support of the talents, and originality of the methods of computing by Zerah Colburn.

which may be proposed to the boy, since such INTERMEDIATE FIGURES NEED NOT BE MENTIONED AT ALL; for it is well known, that, in a company of upwards of one hundred persons, amongst whom were some of the first literary and scientific characters in the kingdom, the following question was distinctly and unequivocally put to the child.—"Can you tell the root of a *perfect* cube number by means of the *two first* and *two last* digits *only?*" He answered "*Yes*:" and that the company might be satisfied that he *clearly understood the nature of the question*, it was put to him again in the following manner: "If a number of 12 *figures* be taken (which shall be a *perfect* cube) and the *two first* and the *two last* figures only be named to you, can you tell the cube root of the whole number?" To this he also replied, *Yes.* He was then tried by *various examples*, which he answered with a facility and correctness *that excited the wonder and admiration of every one present.* Now, Sir, was there in all this any appearance of *a wish to deceive?* any desire to *conceal any thing?* any fear expressed by the boy lest the various questions which were put to him might *lead to a detection of his method?* No, Sir, all was *fair, frank, open,* and *ingenuous*! But I am persuaded, Sir, that what I have stated must be sufficient to convince any unprejudiced person, that Mr. A. has not succeeded in *discovering the method* by which the child performs his operations; and I am therefore led to hope that I may thus counteract the tendency which the publication of Mr. A.'s letter in *your Journal* may probably have had to injure both the boy and his father. I am, Sir, with the warmest wishes for the success of your Journal,

Your most humble Servant,
W. SAINT.

Lower Close, Norwich,
March 13*th*, 1813.

VII.

VII.

METEOROLOGICAL JOURNAL.

		BAROMETER.			THERMOMETER.					
1813.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain	
1st Mo.										
JAN. 24	N E	30·46	30·37	30·415	37	24	30·5			☾
25	N E	30·47	30·45	30·460	36	29	32·5			
26	N E	30·48	30·40	30·440	41	35	38·0			
20	N	30·49	30·47	30·480	39	21	30·0			
2	Var.	30·39	30·37	30·380	32	20	26·0			
2	Var.	30·48	30·39	30·435	34	21	26·5			
3	N W	30.48	30·44	30·460	42	30	36·0			
31	N W	30·50	30·44	30·470	48	34	41·0		0·27	
2d Mo.										
FEB. 1	Var.	30·33	30·24	30·285	41	30	35·5			●
2	N W	30·37	30·32	30·245	41	36	38·5			
3	N W	30·45	30·37	30·410	43	34	38·5			
4	W	30·45	30·29	30·370	41	34	37·5			
5	S	30·29	29·78	30·035	47	36	41·5		—	
6	S W	29·89	29·78	29·835	47	38	42·5			
7	S W	29·98	29·79	29·885	48	37	42·5		—	
8	S W	29·66	29·63	29·645	52	44	48·0		—	○
9	S W	29·88	29·66	29·770	51	35	43·0		0·36	
10	W	30·00	29·88	29·940	46	33	39·5			
11	S	30·00	29·75	29·875	47	35	41·0			
12	S	29·75	29·28	29·515	56	44	50·0		—	
13	S W	29·48	29·37	29·425	57	39	48·0		0·33	
14	S W	29·38	29·27	29·325	52	42	47·0		0·30	
15	S W	29·34	29·27	29·305	52	41	46·5		0·18	☾
16	S W	29·44	29·34	29·390	48	41	44·5		—	
17	S W	29·37	29·30	29·335	52	43	47·5		0·27	
18	S W	29·88	29·37	29·625	52	41	46·5		—	
19	S	29·66	29·60	29·630	56	40	48·0		0·19	
20	S W	29·80	29·66	29·730	53	42	47·5		—	
21	S W	29·70	29·69	29·695	57	49	53·0			
		30·50	29·27	29·957	57	20	40·58		1·90	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

1813. *First Month.* 24. Light clouds and sunshine. 28. Rime on the trees: very misty a. m. clear p. m. 29. Hoar frost: the sky overcast. 30. Misty to the S. a. m. A grey day. 31. Misty a. m. Hoary *Cirrostratus* clouds: rain at night.

Second Month. 1. The *Cumulostratus,* which has not for a long time been exhibited, appeared to-day in large masses. 7. Showers and wind: at sun-set, several large clouds of the modification *Nimbus.* 8. Stormy. 9. A violent thunder gust from the west about 2 p. m. by which considerable damage was done to the roofs and chimnies of houses, &c. This was followed by a series of heavy gales (continuing with a few short intervals of calm and pleasant weather) to the end of the period. The *lunar halo* appeared before several of these, of a large diameter; and, on the 18th, about 11 a. m. there was a brilliant rainbow. The river Lea has considerably inundated the adjacent lands.

RESULTS.

Winds, in the fore part, northerly, with a very dry, dense air, and low temperature: in the latter part, southerly, with a rare and moist atmosphere, and high temperature.

Barometer: greatest observed elevation, 30·50 in.; least 29·27 in.

Mean of the period 29·957 inches.

Thermometer: highest 57°; lowest 20°;

Mean of the period, 40·58.°

Rain 1·90 inches.

The current of evaporation has been again interrupted, and is therefore omitted, in order to be resumed in next report.

These observations will be continued at Tottenham, Middlesex, to which place the observer has removed his residence.

Tottenham,
Second Month, 23, 1813.

VIII.

On the connection between Shooting-Stars and large Meteors, and proceeding both from terrestrial and satellitulæ, in rejoinder to Mr. G. J. Singer.

By Mr. JOHN FAREY, *Sen.*

To William Nicholson, Esq.

SIR,

Phenomena of shooting-stars, and the means of observing them, &c.

BEING in the North of Scotland at the time that your September number appeared, and not having leisure and opportunity since to consult the same until now, I should not otherwise have delayed so long to reply to your correspondent Mr. G. J. Singer, were it only to repel an insinuation with which he concludes, viz. that I had in my 2nd letter to you on shooting stars, *exulted* in your valuable correspondent, Mr. Forster, having been deterred, or in his discontinuing to give his " accurate observations" of meteorological phenomena.

Accurate and sufficient observations on the phenomena of nature I am ever desirous of seeing multiplied as much as possible; not so I confess, those that are so loosely or incompletely made or recorded, as to lead only to the support of what I conceive to be a false hypothesis.

I have elsewhere and repeatedly recommended the multiplying of observations on shooting-stars and meteors, of a nature which does not seem to have occurred to Mr. S. viz. by two or more observers, at several miles distant from each other, each having a well-regulated watch, and a person stationed to read off, and record the observations made, in or near to some constellation previously fixed on by the observers, during a certain time each night, through a long period, the place, direction, and length of course, being recorded by each, with reference to a good planisphere of the part of the heavens fixed on, with which each observer is furnished; the comparative velocity, brightness, interrupting clouds, haziness, light of the moon, &c. &c. being recorded by each observer.

From a pretty complete series of observations thus conducted, in conjunction with my able friend Mr. Benjamin Bevan, now the engineer to the Grand Junction and other canals, and recorded during an hour at least, and oftener two or three on my part, resumed in

Phenomena of shooting-stars, and the means of observing them, &c.

in *every evening* for more than a year; I am compelled to dissent from some of Mr. S.'s facts respecting these phenomena, and entirely so from his manner of accounting for them, by electricity, an agent which we well know was in fashion a few years ago, for explaining a great variety of phenomena.

My observations, above alluded to, decidedly prove, that all which ought properly to be called *shooting-stars*, having only a short and rapid course, and but a small apparent size, appeared only when the air was *clear and cloudless*, in the place of observation, and when only a very small degree of *extraneous light* prevailed, and that the calculated place of many of these, from the best of our concurrent observations, made at 6 miles distant, shewing them to be at 60 to 100 miles distance from us, and at 40 to 50 miles of perpendicular height above the earth's surface, seemed exactly to accord with the short, feeble, and unobtrusive nature of their appearances.

My observations also shew, as I have mentioned, a very regular series, of what I denominate *meteors*, the least of which were nearly as short, quick, and faint in their appearances, as the shooting-stars, to others which were very long in their courses, slow in their motions (apparently) and with corresponding degrees of size and brightness, and attended by explosions and sparks, and small trains, some of the largest of them; but none of which last ever appeared but in clear parts of the sky: and our calculations shewed all of these last, to be far above the height of the ordinary clouds.

Mr. S. mistakes, apparently, in supposing me to have asserted, that the larger class of meteors are not sometimes visible on moon-light nights, and even before the day is quite closed, as I have more than once myself observed, but never *where* clouds appeared at the time, as we almost constantly see of flashes of *lightning*, the only visible *electric* luminous phenomena of the atmosphere, perhaps.

Whence Mr. S. gathered, that satellitic (not planetary or cometary) bodies, move "with *immeasurable* velocity," I am at a loss to think.

As Mr. Forster's authority has been referred to (though very inconclusively, I maintain) in page 34, I cannot help repeating, that if that gentleman, or Mr. L. Howard, ever did, or if they now concur with Mr. S. as to the shooting-stars being an electrical

Phenomena of shooting-stars and the means of observing them, &c.

electrical phenomenon, they would in all probability have resumed the mention of them, and of other undisputed electric phenomena occurring at or near to the same time with them, in their subsequent and valuable papers or reports, in order to shew such their concurrence in Mr. S.'s opinion.

I have expressly mentioned, on different occasions, that it is the largest class of bursting *meteors*, or those having trains of stars after them, which are accompanied by falling meteoric stones, and not the shooting-stars; "*falling-stars*" I conceive to be an improper name for any of these phenomena.

The great comparative scarcity of meteors, and of the stars falling obliquely from them, indicative of their approaching dispersion and end, are sufficiently consistent, and prove nothing against my alleged connection of them with shooting-stars, an extremely frequent, and as I conceive, a periodical phenomenon. And here I would remark, that nearly all the meteors which I or my friends have seen, as well as the shooting stars, have vanished, or ceased to appear, almost at once, in *clear parts* of the sky, where they passed out of the atmosphere, as I conceive, and have not passed behind clouds, as has very commonly been said, in newspaper and other accounts. I have not "travelled" far enough, even in theory, to have said, or even conjectured, *where* the *satellitulæ*, more than the moon or large satellite of our planet, came from; but by no means can I admit the reasonings, that they came recently from the moon's volcanos, or more anciently from a burst planet, conjectured by some, to have given rise to Ceres, Pallas, and Juno.

I entirely dissent from Mr. S.'s 4th alleged fact, at page 36, and from the 4th, 5th, and 6th of those at page 37, as applied to shooting-stars. Mine is not a "*planetary* hypothesis," nor is it opposed, I believe, to either the facts or analogy of these, or other parts of the system of the universe, as now explained on the principles of the universal gravitation of masses, whose motions and present states are within the scope of our cognizance, but not their origins.

I remain, Sir,
Your obedient Servant,
JOHN FAREY, Sen.

12, *Upper Crown Street, Westminster,*
February 22*d*, 1813.

IX.

Account of a remarkable Appearance in the Ice of a Pond in which a man was drowned. (W. N.)

Local situation of a pond in which a man was drowned in Halnaker Park, Sussex.

ON the west side of the road leading from Petworth to Chichester, at the distance of about four miles from that city, stands Halnaker House, formerly the seat of the Earl of Derby, by right of his wife, heiress of Sir William Morley, and more anciently of West, Lord de la War. On the west side of the park, are certain stables, and other buildings, inclosing a farm yard; in which is a pond of about 18 or 20 feet across, and 5 feet deep in the middle. A man was drowned in this pond in the month of November last; and the circumstances attending the discovery of his body were so curious and uncommon, that they occasioned much conversation at the time; but it does not appear that any probable explanation of their cause was pointed out.

Authentic statement of the facts by respectable eye-witnesses.

Very lately I was much gratified by a discussion of this subject in a select company of men of talents and observation, where we had the advantage of the facts being stated by the Rev. James Webber, Chaplain to the House of Commons, who was an eye witness. The clearness and precision with which this gentleman stated the events to us, gave a much more lively interest to the whole: for the narrative in the public prints, which had appeared most remarkable for its strangeness, and perhaps liable to doubt, now assumed the form of an authentic and accurate philosophical incident, capable of being examined and investigated. I did not scruple to request Mr. Webber to favour me with such written minutes as his own recollection, or enquiries among his friends, might afford, in order that I might communicate the same to my readers, with those deductions and remarks which, with the advantage of the conversation before mentioned, I might be enabled to make: and it is to his ready attention to my request, that I am obliged for the following statement, which I have made in my own words from his communication, and those of his brother, Mr. Archdeacon Webber, Vicar of Boxgrove, and the Rev. Mr. Valintine, Domestic Chaplain to the Lord Bishop of Chichester.

The figure of a man was seen on the ice;

About the 14th or 15th of December last, the pond in Halnaker Park, having been frozen over by the hard weather which commenced on the 11th, the figure of a man was observed on the surface of the ice, and upon the event being communicated to several gentlemen in the neighbourhood, they visited the spot, and examined the circumstances most likely to indicate the cause, and its mode of operation.

by a difference in quality. The ice of the figure was clear, hard, and transparent, and the rest brown, soft, and impure.

Snow on the pond; but not on the figure.

The ice was broken, and the body of the man taken up.

The figure corresponded in situation with that of the body,—which had not before risen.

The pond shelves gradually down from its border to its centre, where the depth is about five feet, and the water is discoloured of a reddish brown, by a strong impregnation from an adjoining dung mixen which drains into it: of this colour also was the ice upon the pond, excepting that which composed the figure. This appeared black and was very clear like the purest water, the discoloured water being visible through it; the ice of the figure was extremely slippery and hard, while the rest of the ice was comparatively crumbly and soft. It must also be observed, that a slight fall of snow had covered it all, with the exception of the figure, which by that means became strikingly defined. But at the commencement of the frost, three days before, the snow was seen uniformly covering the whole pond. An opake line surrounded the figure, consisting of ice different in appearance from the rest, and whiter. Fig. 2. plate vi. represents parts of the ice which were taken out of the pond in three pieces, and laid upon the grass near its bank. The body of the man was loosened from the mud at the bottom of the pond by a pitch-fork, and rose at once head foremost with the hat on. It was quite stiff and showed no signs of putrefaction. One of the arms was bent, the hand being inserted under a round Sussex frock he had on; one of the feet pointed upwards, and the other down, and the legs were straight. The figure on the ice corresponded with the outline of the body excepting that the head in the former was terminated abruptly by a line answering to the bottom of the hat. There did not appear any reason to suppose the body had risen beneath the ice; not only because it was discovered fast in the mud, but because the ice was quite flat on both sides, and of the same uniform thickness, namely eight inches. When the ice was held up to the light, the difference of its quality was very singular, the figure being clear and transparent, though greenish, and the other part foul and obscure like the water of the pond: The man's head lay: towards the south-east as was also that of the figure, which was directly over the body. He was recognized to be a traveller or pedlar, who frequently came into those parts, and from the time of his having been missing, he appears to have been drowned on the 30th of November.

This most remarkable and uncommonly curious event, led us into a variety of speculations; several of which were immediately refuted by a fuller statement of the enquiries they indicated. The most obvious of these was that the body might have risen under the ice during the period of its immersion and afterwards subsided. But subsequent information opposed that conjecture; there being no mark nor impression beneath the ice, and very little could have been inferred as to the manner in which such a rising and descending could have affected its colour and transparency. In the search for parallel incidents, some obscure relations were brought forward concerning effluvia, sometimes visible in particular forms over graves and places where the bodies of men in great numbers had been interred after a battle, or over cemeteries where they are placed together without much attention to the closeness of coffins or the manner of covering them from the external air. Other more distinct observations were stated respecting the manner in which dew and hoar frost shew themselves upon the ground; marking particular places where there are drains, or old water courses long filled up, or the bodies of trees or other organized substances, lodged beneath the surface: And along with those facts, the very distinct and limited paths followed by low and dense fogs at their commencement, and often during their whole appearance, and the almost constant recurrence of the same outlines whenever that meteor appears, were also adverted to. To these were added the artificial experiments of Muschenbroek, detailed in his *Essai de Physique*, who exposed plates of earthen ware, china, glass, and metal to the falling dews, sometimes singly, and in other instances, one upon the other; and also those of the like description by Prevost*: in both which the dew attached itself to some of the surfaces and avoided others according to the circumstances of exposure and nature of the material. From all these we seemed warranted to infer that some exhalations or more probably, some energy or power, referable, perhaps, to the phenomena of electricity or of heat, does or may rise, or

Speculations on the event:

Whether the body had risen?

Parallel facts. Exhalations from graves, &c.

Partial deposition of dew and hoar frost;

limited districts occupied by fogs;

experiments on the falling of dew—

seem to shew the agency of a power acting directly upwards;—

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* Philos. Journal, III. 290.

shew its effects by a perpendicular action from beneath the surface of the ground, and is capable of being modified by changes of no great extent in what may be lodged beneath. This power, though imperfectly understood, seemed sufficient to disturb a process so variable and delicate as that of crystallization; which we know is affected by the speedy or slow abstraction of heat or of moisture, the presence or absence of light, the action of tremors of any kind, and several other accidents;—and from a supposed difference in the agency of such a power upon the water immediately over the drowned man, and upon the other water, there appeared good grounds to account in a general way for the difference in their respective qualities.

sufficient to disturb the crystallization of water into ice;—

——and generally to explain the present facts.

More particular investigation.

My own reflections, since I had the pleasure of this conversation, will afford a few additional remarks, which may give a more specific form to this explanation. I am disposed to think that the effect was caused by the developement of heat in the body, occasioned by the putrefaction or chemical change which must have taken place, though it may have proceeded very slowly, on account of the coldness of the water, and the want of communication with the external air. This would occasion a small degree of expansion in the water, in contact with the body, and cause that fluid to ascend perpendicularly to the surface; where it would spread itself (thinly) on all sides, and when cooled would descend near the circumference, as is daily seen with fluids heated from beneath. Or, in other words, the particular mass of water, in this stagnant pond, which was perpendicularly over the corpse, and consequently had the same precise outline, as to all its horizontal sections, would be a little warmer than the rest. And though this circulation would be a little modified by the expansion to which water is subject, when cooled below 40°, yet this would scarcely in any case affect the result, and not at all, if the freezing came on suddenly. It appears from our meteorological tables that it did so, on the 11th December.

The body must have been slightly heated by its chemical change; and would warm the water above it; which would ascend, &c. having the outline of the figure.

The water exterior to the figure would freeze first confusedly, and form opake foul ice.

Now these simple facts might, and probably did, occasion the great differences in the ice. For the external water, being originally colder at the commencement of the frost, would sooner be cooled down to the freezing point, and begin to congeal over its whole surface, while the other part would remain fluid. The congelation being sudden, and from all points at once, would be of that kind which chemists call confused crystallization; forming an opake mass, and including the impurities of the liquid in its substance. But the water over the body being warmer, would not congeal at so early a period, except with regard to a few spiculæ, which would shoot into it from the surrounding ice, and form the white border of the figure; and when at last its surface became gradually cooled down to the freezing point, this water would freeze, not all at once, but by a slow crystallization, from the edge inwards; which is the very process for making clear ice, used by Achaed, in his electrical experiments, and is known to exclude all air bubbles, and mechanical mixtures of impurity from the crystals.

The water over the body would freeze more slowly, and form a white border to the outline, and clear hard ice within.

These effects, indeed, suppose a concurrence of favourable circumstances which may rarely take place, but really appear to have met in this uncommon case, and seem to support the preceding explanation even in the minuter particulars. Thus the abrupt termination of the head of the figure, agreeing with the lower part of the hat, leads us to an inference, that the warmed water either found its way from under the hat, instead of rising from its upper border, or that the felt acted as a bad conductor of heat, to the surrounding water: and the remarkably smooth slippery face of the ice of the figure, clear of snow, while the rest of the ice was rougher and covered with snow, should indicate that the rough ice was sufficiently cold to protect its snow from thawing by the day temperature, which in fact was *above* freezing; while the ice of the figure, though covered with snow on the 11th or 12th, was not so cold as to prevent that snow from melting in the day time, and again freezing in the night, with that very smooth and slippery surface which is noticed in the description.

The effects required a concurrence of circumstances, which appear to have taken place; and are confirmed by the minor incidents.

X.

Of the Caldeiras or hot fountains of the Furnas in the Island of St. Michael, one of the Azores.

PLATE VII. is taken, by permission, from "the History of the Azores," an entertaining work lately published, respecting a part of the globe but little attended to. The author's account of this phenomenon, states, that the columns of water are of very considerable diameter, and are thrown up to the height of about twelve feet. He did not take the degree of the heat, but from its effects it appears to have been equal to that of boiling. Sulphur rises in vapour through the surrounding ground, and the water itself is strongly sulphureous.

My correspondent R. B. has promised a Memoir upon the Theory of Hot Springs and Fountains, which I shall be glad to receive, and particularly that of the two alternately intermitting springs of Iceland, described by Sir G. S. Mackenzie.

SCIENTIFIC NEWS.

Geological Society.

March 5th, 1813.

The Right Hon. the Marquis of Landsdown,
The Right Hon. Charles Long, M.P.
William Clarke, Esq. Trinity College, Cambridge,
were severally elected members of the Society.

Two letters from Mr. Webster, Draughtsman, and Keeper of the Museum to the Society, addressed to L. Horner, Esq. were read.

In the first, Mr. W. states, that, during a late examination of the Isle of Wight, made by him for Sir H. Englefield, he discovered a series of calcareous strata of later formation than the chalk, especially characterized by containing fresh water shells. From this circumstance he was led to suspect a correspondence between this formation and the Calcaire d'eau douce, which has been described by Brugniart and Cuvier as forming some of the strata in the basin of Paris; which conjecture was confirmed by a comparison of the fresh water fossils of the Isle of Wight with those of the French strata, which were given by M. Brugniart to the Count de Bournon, and by him have been deposited in the cabinet of the Geological Society.

In consequence of this interesting discovery, the attendance of Mr. Webster at the Society's apartments was for a time dispensed with, that he might re-examine the Isle of Wight and its vicinity. Accordingly, his second letter is dated from Freshwater, in the Isle of Wight, March 3d. In this he states, that he has succeeded in obtaining some very desirable sections of the strata, and an abundant collection of specimens. He is inclined to think that there are two freshwater formations,

tions, with a marine formation between them. The lowest freshwater formation consists of beds of sand and marl, with numerous fragments of the Limnea of Lamarck, and of two species of Planorbis: the interposed marine deposit is of blue clay with Venuses, oysters, and various turbinated shells: and the upper freshwater formation consists of a calcareous rock, inclosing numerous and very fine specimens of the Limnea and Planorbis. Some of this last stratum is very friable, being only marl; other parts are extremely hard, and the uppermost part of it has a porcelaneous character. Mr. W. has not discovered any bed of gypsum, nor silicous concretions.

A paper by Dr. Mac Culloch on the Granite Tors of Devonshire and Cornwall, accompanied by three drawings, was read, and thanks were voted for the same.

The object of this communication is to show the process followed by nature in the destruction of the Granite rocks of Cornwall and Devonshire, and to explain how this process is dependent upon a particular mode of aggregation of the materials of which this rock consists, and which can be satisfactorily observed only during the progress of its decomposition.

Dr. M. first treats of the Granite which forms that promontary near the Land's-end on which the Loggan rock is situated. Its length is about 200 yards, and the entire rock of which it is composed is traversed by numerous vertical and horizontal fissures, thus dividing the mass into a multitude of cubical and prismatic blocks. The Loggan rock itself, which is the subject of one of the drawings, appears to be one of these blocks in a state of decay, but still occupying its original site. Its general figure is irregularly prismatic and four-sided, with a protuberance on the lower surface on which it is poised. The breadth of the apparent surface of contact between this protuberance and the rock that it rests upon, is about a foot and a half, but its figure being cylindrical, and not spheroidal, the motion of the stone is limited to a vibration in one direction. The utmost force of three persons (according to Mr. M.'s trials) is only capable of making its exterior edge describe an arc, the chord of which, at six feet distance from the centre of motion, is three quarters of an inch. A force of a very few pounds, however, is sufficient to begin and maintain a very visible degree of vibration. Even the wind, when blowing on its exposed western surface, produces this effect very sensibly. Its weight, as estimated from its dimensions, and the specific gravity of granite, appears to exceed 65 tons.

The subject of the second drawing is the *Cheese-wring*, which occupies the highest ridge of a hill to the north of Liskeard. It is an irregular column, about fifteen feet high, composed of five stones, the upper ones of which are so much larger than the rest as to overhang the base on all sides. The angles and external borders of these stones are considerably rounded by the effect of decomposition; and there is no doubt that in process of time, this disintegration will proceed so far, that the balance of the pile will be destroyed, and its ruins will not be distinguishable from the other bowldens, with which the tops of all the hills in this vicinity are overspread.

The third drawing represents the Vixen Tor on Dartmoor. Almost all the granite of Cornwall and Devon, like that of the Land's-end, is divided by fissures into masses more or less approaching a cubical form. If a rock of this kind, nearly level with the surface of the soil, is examined, the fissures will be found to be a mere mathematical plane, and the angles formed by their intersection will be sharp and perfect.

If we then turn our attention to granites, which, from their greater elevation above the soil, appear to have been longer exposed to air and weather, we may observe a gentle rounding of the angles, such as is exhibited in the Vixen Tor. By degrees the fissures become wider, and the blocks, which were originally prismatic, acquire an irregular curve-lined boundary, resembling those which form the Cheese-wring. If the centre of gravity chances to be high, and far removed from the perpendicular of its fulcrum, the stone falls from its support, and becomes rounder by the progress of decomposition, till it assumes one of the various spheroidal figures, which the granite bouldens so often exhibit.

These fissures, and the rounded form which the cubical blocks acquire by decomposition, Dr. M. is inclined to attribute to the original structure of the rock. In this, as in basalt, crystallization appears to have been begun in distinct, and more or less distant points, from each of which it proceeded, forming thick concentric lamellæ, till at length the exterior shells of adjacent concretions came in contact, but were incapable of mutual penetration. The outer lamellæ are the least hard and dense, and therefore yield the easiest to the various causes which occasion the disintegration of rocks.

March 19*th*, 1813.

Mr. Webster exhibited specimens of the rocks containing freshwater shells recently discovered by him in the Isle of Wight.

A paper on the rocks of Clovelly, in Devonshire, with illustrative drawings, by the Rev. I. J. Conybeare, was read, and thanks were voted for the same.

The fishing town of Clovelly is situated in a narrow ravine on the north coast of Devon, about 22 miles west of Ilfracombe. The shore is precipitous, being formed of cliffs about 130 or 140 feet in height, intersected by narrow alternations of Granwarke and Granwarke slate, curved and contorted in the most capricious way imaginable. No organic remains were observed in them, nor any foreign minerals, except opake white quartz, forming numerous veins. Nearly the whole of North Devonshire is composed of the rock just described, which is locally distinguished into *Dunstone and Shllat*, the latter being the slaty Granwarke, and the former the compact: it is always very irregular in its stratification, is destitute of metallic veins, alternates with transition limestone, and, where it does so, occasionally contains organic remains. It also, in one instance at least, alternates with thick beds of a kind of culm: its veins besides quartz occasionally offer calcareous spar. Killas, which Mr. C. is inclined to regard as a variety, not of nuca-slate, but of clay slate, is contorted in its stratification only in the neighbourhood of the Granwarke, is traversed almost through its whole extent by frequent veins or dykes of a porphyritic rock which does not pass into the Granwarke, contains sometimes topaz, and not unfrequently garnet: its veins are often filled with chlorite, mica, and crystallized filspar, and also contain tinstone, grey cobalt ore, &c. These characters, in the opinion of the author of this paper, form a sufficient mark of distinction between the Granwarke and the Killas of the West of England.

A paper on the Isle of Staffa, by Dr. Mac Culloch, was read, and thanks were voted for the same.

The circumference of this island is about two miles: it forms a kind of table land of irregular surface, gently sloping to the N. E. and is bounded on all sides by steep and generally perpendicular cliffs from 60 to 70 feet above high water mark, the greatest elevation in the island being about 120 feet.

The entire island is a mass of basalt, but a bed of sandstone is said to be visible at low water on the western side.

The basalt presents two varieties, the columnar and amorphous, the latter of which is generally amygdoloidal, containing zeolites. In the columnar variety lamellar stilbite is occasionally found filling the intervals of approximate columns, and sometimes, though rarely, in the substance of the smaller and more irregular columns.

On the south-western side of the island there appears to be three distinct beds of basalt, the lowermost of which seems to be amorphous. The next bed from 30 to 50 feet thick, consists of those large columns which form the most conspicuous feature of Staffa. The upper one appears at a distance to be a mass of amorphous basalt, but on closer inspection is found to consist of small columns, often curved, laid and entangled in every direction. The columnar basalt of Staffa is by no means so regular as that of the Giant's Causeway; but in return it presents many beautiful specimens of bending columns, which do not occur at the latter place; of these the most remarkable form a conical detached rock called Budchaille, or the Herdsman. Besides the great cave are two smaller ones, which being accessible only by a boat, and in perfectly calm weather can very rarely be examined.

The surface of the island is in some places overspread with a bed of alluvial matter, containing rounded fragments of granite gneiss, mica slate, quartz, and red sandstone, together with a few rolled pieces of basalt. As there is at present a considerable extent of deep sea between this bed and the nearest primitive rocks of Jona, Coll, Tirce and Mull, it becomes an interesting subject of speculation to inquire into the conditions requisite to account for this fact, there seems to be, either that a declivity sufficient to allow of the transportation of rounded fragments has formerly existed, sloping upwards from the present level of Staffa to some primitive mountains, which no longer exist, or that the whole island of Staffa itself has been raised to its present elevation from the bottom of the deep sea by which it is now surrounded.

Incombustible Cloth.

It appears, that the ancients had a method of making incombustible cloth of amianthus, which, notwithstanding the flexibility of its fibres has generally been considered as too brittle to be worked without a mixture of some other staple, such as flax, or cotton, to be afterwards burned out. Madame Perpenti has succeeded in working it with facility alone, after several trials from the writings of ancient authors.

Much depends, of course, upon the quality of the article itself. Her process consists, in softening the amianthus in water, beating it, rubbing it, and dividing with a double comb, with fine steel points. It is remarkable, that the fibres thus obtained are much longer than the solid piece, and may be had of the most extreme delicacy for fine fabrics. They are said to be as strong as those of silk or linen.

She has manufactured paper of this material, making use of gum to give consistence to the pulp. If an incombustible ink be required, the oxide of manganese would present itself as a preferable ingredient.

To Mr. Nicholson.

Sir,

Your correspondent O, in No. 158, will oblige a constant reader of your Journal to remove the following ambiguities in the extraction of the square and cube roots:

Sir,
Your obedient Servant,
B.

If the terminating figure of the power given be 1 or 6, t last figure of the *square* root may be 1 or 9, in the first, and 4 or 6 in the second case, how is the right figure to be ascertained ?

Again, 25, 672, 375—I want the *cube* root, according to the rule given A=5 and B=5—how is the process given by

O to be applied here ? for from - - - 25
the millions given, if you deduct - - - 8
the cube of 2, the first figure of - - - —
the root, you have - - - 17

which is to be divided by the *two first figures* of thrice the square of 2—but 2×2×3=12 (17)

12
—
5

1 is the quotient, but 9 is the number sought : how is this ?

Dr. Henry has requested the Editor to state, that Professor Bezelius, in his able sketch of the present state of animal chemistry, recently published, has committed an error in ascribing to Dr. H. the suggestion of the trial of magnesia in calculous diseases ; and that the merit of the hint which led to the successful experiments of Mr. Brande, belongs entirely to Mr. Hatchett.

Mr. Snart, Mathemetical Instrument Maker, 215, Tooley Street, Southwark, has shewn me by a letter, that he invented the Chondrometer described in our last, and supplies the scale-makers and others with them, who have no pretensions to the contrivance. He has likewise had the candour to remark, that he subsequently found, that the same had been used several years before.

A

JOURNAL

OF

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AND

THE ARTS.

SUPPLEMENT TO VOL. XXXIV.

ARTICLE I.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor J. BERZELIUS.

(*Continued from p.* 246.)

The proportions of oxigen and of sulphur in antim. compounds are the same as with other metals.

IF we admit that 100 p. of antimony form the white oxide with 27·8 p. of oxigen, and the sulphuret with rather more than 37 p. of sulphur, this result corresponds very well with these data, and proves that the white oxide contains 1½ times as much oxigen as the fusible oxide, and that the sulphur in the sulphuret is to the oxigen in the fusible oxide in the same proportion as we have found with the other metals. For if, in this case, the white oxide be composed of 78·25 p. of metal, and 21·75 of oxigen, this last, in the experiment I have mentioned, was replaced by 29 p. of sulphur—that is to say, 7·25 p. more than the weight of the oxigen; and these 7.25 p. making precisely one-third more than the weight of the oxigen, and the antimony is to the sulphur, in this experiment, as 100 : 37·07.

If any one among the experiments had produced a very decided result, we might easily have calculated the others from it; but

but at present we must be content with the mean or average numbers.

5. Yellow oxide of antimony. Antim. digested with nitro-mur. acid or fuming nitrous acid, the dried mass is yellow.

5. *Yellow oxide of antimony.* If antimony be digested with the nitro-muriatic acid, or with fuming nitrous acid, and the solution be evaporated to dryness, and then sufficiently heated to drive off the acids without igniting the mass, a yellow powder is obtained, which is often crystalline, and, if strongly heated in the fire, becomes white, and consequently did not owe its (yellow) colour to an adulteration with the red oxide of iron.

100 p. of metal gave 130 oxide.

When I made this experiment in a glass phial, weighed, and made use of the fuming nitrous acid, I obtained as much as 131 p. of a yellow crystalline powder; but when I had oxided the metal by means of the nitro-muriatic acid, the result did not exceed 129 or 130 p. of oxide from 100 p. of metal, and new cohobations of acid upon the yellow oxide did not increase the weight. The addition of 30 or 31 p. of oxigen does not agree with any proportion of those which I had before found, and I began to suspect a combination of two different degrees of oxidation had been formed, and that this combination could not be farther oxided by means of acids.

supposed to be a compound of two different degrees of oxidation,

I shall not mention the various unsuccessful experiments I made to clear up this intricate subject, but shall speak only of those which afforded more satisfactory results.

Antim. oxided by red oxide of mercury,

I mixed powdered antimony with the red oxide of mercury, prepared from very pure mercury, and heated the mixture in a glass retort. At a certain temperature it took fire, and the mass became red. I continued the heat as long as any mercury was condensed in the neck of the retort; after which, at length, there remained a deep olive-coloured powder. I heated this with great care in a glass capsule, by which treatment it lost its olive colour, and left a straw-coloured powder in the capsule.

gave straw-coloured powder.

The yellow oxide loses one fourth of its oxigen by heat, and becomes white oxide.

One hundred parts of this powder very strongly heated in platina crucible, left, in different experiments, from 33·5 to 33·75 parts of white oxide of antimony. In order to determine the nature of what had been dissipated by the heat, the experiment with the yellow oxide was repeated in a small glass retort, with a small apparatus for collecting the gas. When the retort began to be strongly ignited, a gas was developed, and when this ceased the retort was removed from the fire. The residue was white oxide, and the gas very pure oxigen. These experiments prove, that the yellow oxide is decomposed by the heat by losing part of its oxigen ; and as 93·75 p. of white oxide contain 20·24 of oxigen, it is clear, that the yellow oxide loses, on this occasion, one-fourth of its oxigen, and that it contains 1⅓ as much oxigen as the white oxide, and twice as much as the fusible oxide (oxidum stibiosum.)

Other methods of producing yellow oxide (a) by mere heat;

There are several other methods of producing the yellow oxide *(a.)* If into a long-necked tubulated phial a certain quantity of metallic antimony be put, and heat be applied to a cherry redness, and continued four or five hours, the antimony gradually combines with oxigen ; a small quantity of white oxide sublimes into the neck of the glass, another portion escapes out of the aperture ; but the greatest part of the oxided metal forms a yellow crust round the border of the fused metal. In this form the yellow oxide is not crystallized ; it has much tenacity, and is difficult to break.

(b) by combustion with nitre.

(b) If one part of antimony in powder be burned with 6 p. of nitre in a red hot crucible, the burned mass is then decomposed with nitric acid, which leaves a white powder not dissolved. This powder is a combination of the yellow oxide with water, which may be driven off by heat over a spirit lamp. The oxide is then left of a very clear and fine yellow colour.

Degrees of oxidation stated in series 1,—,4, 6, 8.

The degrees of oxidation of antimony may therefore be expressed by the series 1, 1½, 2 : but we have not comprehended the suboxide in this series, which contains less oxigen than the first of the series. If, from what I have endeavoured to make probable with regard to sulphur and arsenic, the quantity of oxigen in the suboxide be one-sixth of that in the white oxide, or one-fourth of the fusible oxide, the series will be 1,—, 4, 6, 8. If, on the contrary, future experiments should prove, that the oxigen in the suboxide is only, in fact, half that of the fusible oxide, the series will become 1, 2, 3, 4.

Although it is very clear, that the middle numbers must be deficient in precision, particularly when, at the same time, they are not founded upon good experiment, yet we must, amidst results so little fixed, content ourselves with an approximation of this nature. And by assuming, that the oxidum stibiosum is composed of 100 parts of metal, and 18·6 p. of oxigen ;

oxigen ; and that the suboxide contains with 100 parts of metal one-fourth part as much, the oxides of antimony will be composed as under.

Table of the proportions of oxigen in the four oxides of antimony.

	Met.	Oxigen.	Metal.	Oxigen.	Metal.	Ox.
The suboxide	100	4·65	96·326	3·174	2150·65	100
The fusible ox.	100	18·60	84 317	15·683	537·63	100
The white ox.	100	27·90	78· 19	21·810	358·41	100
The yellow ox.	100	37 20	72·85	27·150	268 81	100

Yellow oxide by n. acid appears to be composed of the white and yellow oxides, in portions containing equal quantities of oxigen.

Now, if we calculate the composition of the yellow oxide obtained by the action of nitric acid upon antimony, we shall find it such as it would be if composed of the white oxide and the yellow oxide, in such proportions as that each of them should contain an equal quantity of oxigen*.

[*The following annotation, on account of its length, is continued in the body of the Memoir.*

Other instances of compounds of two oxides, each of which appears to contain the same quantity of oxigen; and serve as the bases of compound salts.

* In proportion as our researches shall be extended, we shall find abundance of facts of this nature. In my first essay upon determinate proportions, I had the notion, that the difference of colour in precipitates, caused by the alkalis in the solutions of iron depended on a combination of two degrees of oxidation of iron, which probably afforded salts with excess of base, and I gave examples in the nomenclature by the prussias ferroso-ferricus, sulfas ferroso-ferricus, &c. One circumstance, which strikingly proves these notions, is found in the phosphas ferrosus. By precipitating sulphas ferrosus by means of a neutral phosphate, a white precipitate is obtained; and the same happens by precipitating the phosphas ferricus. But the first, the phosphas ferrosus, acquires a fine blue colour by the washing, which does not happen with the latter. In fact, a blue precipitate is obtained by the alkaline phosphates, in a mixture of the sulphas ferrosus and sulphas ferricus. The blue phosphate of iron is, therefore, a salt with double basis, containing the two oxides of iron, representing two different bases, and it constitutes a phosphas ferroso ferricus. The phosphas ferrosus, on the contrary, is white, as well as the phosphas ferricus.

Metals possess this habitude.

I have reasons to think, that several metals, viz. manganese, uranium, cerium, and mercury, have the property of producing salts with double bases, by combining their two degrees of oxidation. But it is, no doubt, still more interesting to find that the same kind of combinations likewise exists between the different degrees of oxidification of the same radical. It is known, that M. Gay Lussac, by extremely interesting researches, has found that 100 p. in volume of azote gas combine with 50 p. in volume of oxigen gas, to form the nitrous oxide,—that with 100 p. of oxigen they form nitrous gas, and with 200 p. nitric acid. The series would here be 1, 2, $3\frac{1}{2}$, 4, which is contrary to all analogy with other bodies. The irregularity of this series did not escape the attention of this learned chemist; but he endeavoured to remedy it by considering nitrous acid as composed of three volumes of nitrous gas, and one volume of oxigen gas. The analogy with the other oxides combined with the results of my experiments on the nitrites is here, in fact, 1, 2, 3, 4; and that the nitrous oxide, such as it is found in the nitrites, is composed of 100 p. in volume of azote, and 150 p. in volume of oxigen. But if we consider that, probably, neither the nitrous nor the nitric acid are capable of existing together in insulated states, as is also the case with many other acids; and that when nitrous acid is produced, the result would be a combination of the nitrous with the nitric acid as oxided bodies, and in such proportion that each shall contain an equal quantity of oxigen, the quantity of oxigen absorbed will be precisely what was found by M. Gay Lussac.

The same doctrine applied to the bases of acids; e. g. azote and oxigen.

(Here the annotation ends.)

A very interesting question remains to be considered respecting the oxides of antimony. What is their chemical nature? Is the yellow oxide a superoxide, or has it the properties of an acid? The following experiments will prove, that the yellow oxide, as well as the blue, possess the characters of acids, and that the yellow oxide may be considered as an acidum stibicum, and the blue as an acidum stibiosum.

The yellow and the blue oxides of antimony have the characters of acids.

(a) Combinations of the acidum stibicum with saline bases, stibiates.

I burned 10 grammes of antimony in powder, with 60 grammes of pure nitre in a silver crucible, and the mass was heated during an hour in the highest temperature the vessel could support. I pulverized the white mass which was thus obtained, washed it with cold water as long as any nitrate of potash

Combinations of ac. stibicum (or yellow oxide) with saline bases. *Stibiates.* Stibiate of potash.

potash could be so dissolved; a white powder was then left, which I dried upon blotting paper. One part of this powder boiled for some hours with water was dissolved, and I passed the solution through the filter. The filtered liquid had a slightly bitter taste, rather metallic. It restored the blue of turnsole paper, which had been reddened by a weak acid, and the smallest quantity of a diluted acid caused a precipitate which was not redissolved in the fluid. The precipitate afforded by the acetic acid, well washed, was white and tasteless; but it reddened turnsole paper, although the filtered solution had not yet lost the property of acting like a weak solution of alkali. The white precipitate caused by passing a current of carbonic acid gas through the alkaline liquor, had the same property of reddening turnsole paper; a property which consequently belongs to the precipitated oxide, and cannot be owing to the combination with the acid, which served to precipitate it. The dry precipitate did not, in several months, lose the property of re-acting like an acid upon vegetable colours. But when I heated it in a retort over the flame of a spirit lamp, a quantity of water was disengaged, which did not redden them, and was very pure; and the yellow oxide which remained had lost the property of reddening turnsole paper.

deprived of its potash leaves an hydric stibiate.

One part of the saline solution, evaporated in a silver crucible, left a white mass. I decomposed this by digesting it with diluted nitric acid, by successive operations, to extract the potash. The undissolved oxide was white. I washed it with much water, and at last dried it at the temperature of boiling water; and then I heated certain portions, previously weighed, in order first to drive off the remaining water, and next the oxigen, until the white oxide only should remain. In several experiments 100 parts left 88·3, 88·7 and 88·9 p. of white oxide. The quantity of oxigen combined with 88·7 p. of the white oxide, in order to form the yellow oxide, is, from what we have determined, 6·45; and therefore the quantity of water was only 4·78. But 95·22 p. of yellow oxide contain 25·81 p. of oxigen, and 4·78 of water contain 4·218; now, 4·218 × 6=25·3 It follows, therefore, that the white powder not dissolved by the nitric acid, is an *hydrique stibiate*, that is to say, a combination of stibic oxide, with water, as its base, and in which the acid contains six times as much oxigen

The nitric acid by which the stibic acid was separated from the saline mass, contains nitrate of potash. This saline mass was, therefore, in fact, a stibiate of potash.

(*To be continued.*)

II.

Inquiries relative to the Structure of Wood, the specific Gravity of its solid Parts, and the Quantity of Liquids and elastic Fluids contained in it under various Circumstances; the Quantity of Charcoal to be obtained from it; and the Quantity of heat produced by its Combustion. By COUNT RUMFORD *F. R. S. Foreign Associate of the Imperial Institute of France, &c.*[*]

SINCE the days of Grew and Malpighi, there have been but few regular inquiries into the structure of wood. The science of botany has, indeed, taken an excursive range; and the indefatigable zeal of modern naturalists, who have travelled over all the known world, has made us acquainted with an astonishing number of plants, unknown before in Europe, and therefore called *new*, by which our gardens and apartments are embellished with a profusion of gay flowers; but still the knowledge of the vegetable economy is scarcely at all advanced. The circulation of the sap in plants is still a subject of dispute, and the causes of its ascension are very imperfectly known. The specific gravity of the solid parts which form the wood of plants, is unascertained, and, by consequence, the proportions of solids, of liquids, and of elastic fluids; the component parts of a plant, with the variations to which they are subject in different seasons, are matters of which we are still ignorant.

It is, indeed, known, that the wood of a tree remains and preserves its primitive form after it has been converted into charcoal; but no one has explained this extraordinary phenomenon, very little attention having been paid to it.

An earthen vessel becomes hard and brittle in the potter's furnace; the vessel shrinks during the operation of baking, but it undergoes no alteration of shape. This phenomenon is easily accounted for : the water which distended the particles of the clay, kept them at a distance from each other, and rendered the mass soft and flexible, having been expelled by the power of the heat, the several particles contract themselves together, and form a hard and brittle body, though the clay remains the same before and after the operation.

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[*] Read at the sittings of the first class of the Institute, September 28, and October 5, 1812.

Is it not possible that wood is converted into charcoal by a similar process? For, either the charcoal is already formed in the wood, or the wood being decomposed, the charcoal is formed of its elements, or a part of them. But is it not evidently impossible that the elements of a solid body should be so totally deranged as to separate them entirely from each other without destroying the form or figure of the body ?

In the sequel of this paper it will be shewn, that the specific gravity of the solid parts of any kind of wood is very nearly the same as that of the charcoal obtained from it, a circumstance that gives a great degree of probability to the hypothesis, that the two substances are identically the same.

But I do not mean to amuse the Class with a detail of my own conjectures ; it is to my experiments and their results that I now claim the honour of calling its attention.

I was by accident first induced to enter upon this examination and inquiry into the structure of wood. In the course of a long series of researches upon heat, I wished to determine the quantities of that element produced by the combustion of different kinds of wood ; but I had scarcely began the inquiry when I found, that in order to procure satisfactory results to my experiments, it was indispensably necessary to obtain a better knowledge of wood itself; and, therefore, I immediately devoted myself to the study.

My first aim was to determine the specific gravity of the solid parts which compose the fabric of the wood, in order afterwards to determine the quantities of sap or water contained in wood under various circumstances.

Having found, that very thin shavings filled with sap, or even with water, could be thoroughly dried in less than an hour, without injury to the wood, in a stove kept at a higher temperature than that of boiling water, or at about 50°

of Fahrenheit's scale, (= 260° French) I determined on using shavings of this description in my experiments.

SECTION I.

Of the specific Gravity of the solid Parts of Wood.

Specific gravity of the parts of wood, &c.

I began with the wood of the lime-tree, of which the texture is very fine and regular. From a small board, five inches long and half an inch thick, very dry, I took a quantity of thin shavings with a very sharp plane. These were exposed for eight days in the month of January, upon a table in a large room, not otherwise occupied, in order that they might attract from the atmosphere all that moisture which, as an hygrometric body, they were capable of imbibing. The temperature of the room was about 46 F.

Ten grammes (154·5 grs.) of those shavings, laid on a China plate, were placed in a large stove made of sheet-iron, and there exposed to a regular heat of about 245° F. for two hours, in the course of which time they were frequently taken out and weighed, in order to observe the progress of their dissication. When they ceased to lose weight, the operation was stopped; when perfectly dried, their weight was 8·121 grammes.

By previous trials with my apparatus, I had learned, that if the stove was too much heated, the shavings became discoloured, which is always indicated by the emission of a particular odour, very readily to be perceived; but, by a careful regulation of the fire, this accident may be avoided, and the shavings be thoroughly dried without injury, or even subjecting them to any sensible alteration.

I concluded that they had not undergone any change, because, upon again exposing them to the atmosphere, they regained the same weight which they had, under similar circumstances, prior to their being dried in the stove.

Being thus possessed of the weight of my shavings, as well under exposure to the air as in a dried state, which latter I could not but look upon as being perfect, it only remained to ascertain their weight in water when all their vessels and pores were completely filled with that liquid, to enable me to determine

Specific gravities of wood, &c.

amine the specific gravity of the solid parts of this wood, which was accomplished without difficulty by the following process:

A cylinder copper vessel, ten inches in diameter, and as many deep, was filled with water from the Seine, previously well filtered, and being set upon a common chafing dish, was made to boil for some time, to expel the air contained in the water. The shavings were then thrown into the boiling water, and kept in that state for an hour. The water was not long in filling the vessels and pores of the shavings, from which it dislodged the air contained in them; so that the wood, specifically heavier than the water, was precipitated to the bottom of the vessel, and there remained.

When the vessel was removed from the chafing-dish, the water was suffered to cool to the temperature of 60° F. and then plunging in both hands, I placed (under the water) all the shavings in a cylindric glass vase, whose weight I had previously ascertained, which was suspended in the water by a silken cord, fastened at its other extremity to the arm of an accurate hydrostatic balance.

On weighing the shavings in the glass vase thus immersed, I found their weight equal to 2·651 grammes.

As the shavings, while dry, weighed 8·121 grammes, in the air, and 2·651 grammes in the water, they must have lost 5·47 grammes of their weight in the latter; consequently, they must have displaced 5·47 grammes of water; and the specific gravity of the solid parts of this wood must be to that of the water, at the temperature of 60° F. as 8·121 to 5·47, or as 14·846 to 10·000.

It may, perhaps, excite some surprise, that the solid parts of so light a wood as that of the lime-tree should be heavier, by nearly one-half, than water, taken in equal bulks. But this surprise will, without doubt, be increased when I declare, that the specific gravities of the solid parts of all kinds of wood are so nearly alike, as almost to induce a belief, that there is the same identity in the ligneous substance of all sorts of wood, as in the osseous substance of all species of animals.

I procured, from a joiner's work-shop, dried wood of the eight following species, viz. poplar, lime, birch, fir, maple, beech, elm, and oak; and had them cut into small boards, five inches in length, and six inches broad, from each of which

Specific gravities of wood, &c.

I planed off some thin shavings, and exposed them to the air for eight days, in the month of January, in a large room, where the temperature, which varied but little, was about 40° to 45° F.

When these shavings had acquired their ordinary degree of dryness under existing circumstances, ten grammes of each sort were weighed off, and being laid separately in China plates, were thoroughly dried in the stove.

On being taken out of the stove, they were again weighed, and then thrown into boiling water, to expel the air from their pores, and to moisten them thoroughly. When they had boiled for an hour, they were suffered to remain in the liquor till it was sufficiently cool; and after they had been weighed in the water, the specific gravity of their solids was calculated in the usual way.

The following table gives the details and results of this inquiry.

Species of wood.	Weight.			Specific gravity of the solid parts of the wood.	Weight of a cubic inch of the solid parts of the wood.
	Exposed to the air in a room in winter.	Thoroughly dried in a stove.	In the water at 60° F.		
	grammes		grammes		grammes
Poplar	10	8·045	2·629	14854	29·45
Lime	10	8·121	2·651	14846	29·40
Birch	10	8·062	2·632	14848	29·44
Fir	10	8·247	2·601	14621	28·96
Maple	10	8·137	2·563	14599	28·93
Beech	10	8·144	2·832	15284	30·30
Elm	10	8·180	2·793	15186	30·11
Oak	10	8·336	2·905	15344	30·42
			Water	10000	19·83

The specific weight of the solid matter which composes the fabric of these woods is so nearly alike in them all, that the small variations to be observed in the different experiments, may, perhaps, be accounted for otherwise than by supposing the ligneous substance to be essentially different in the several species.

The charcoal obtained from the various kinds of wood, if carefully prepared, has no sensible difference; and all the seer-woods give nearly the same chemical results when treated in the same manner. Hence, without doubt, we have good reason to suspect, that the ligneous substance of all woods is identical. But without stopping to discuss this question at present, I shall endeavour to elucidate another, no less interesting, and which yields results more satisfactory.

Section II.

Of the Quantities of Sap and Air discovered in Trees, and in Seer-woods.

Sap, &c. in different woods.

Greeve and Malpighi discovered in plants certain vessels, which they suspected to be destined for the reception of air; and many physiologists have supposed, that the air found shut up in the vessels of plants, which (if it be really confined there) would necessarily cause a reaction upon the neighbouring vessels, with an elastic force, as variable as the temperature to which this elastic fluid is exposed, and might probably contribute to the circulation of the sap.

It would, doubtless, be an interesting question to determine precisely the quantity of air contained in plants in different seasons, and under various circumstances. By examining the variations to which this quantity of air is subjected, and combining them with other simultaneous phenomena, we might hope to make some discovery which may assist us a little to elucidate the profound obscurity that at present conceals this part of the vegetable economy.

The specific gravity of the solid parts of a plant being known, it becomes very easy to determine, in every case, the quantity of air contained in its vessels and pores.

The following example will render this position perfectly clear:

An oak, in complete health, in a growing state, was cut down on the 6th of September, 1812. A cylindrical piece, six inches long, and rather more than an inch in diameter, taken from the middle of the trunk of this young tree, three feet above the earth, weighed, when full of sap, 181·57 grammes.

Upon plunging this piece of wood into a cylindric vessel about 1½ inch in diameter, and 6¼ inches in height, filled with water at the temperature of 62° F. it displaced 188·57 grammes of the

Sap, &c, in different kinds of wood.

the water*; whence I conclude with certainty that this piece of oak, filled with sap, possessed a bulk equal to 9·5093 cubic inches, that its specific gravity was 96515, and, consequently, that a cubic inch of it weighed 19·134 grammes.

When the piece of wood had been reduced to the shape of a small board, about half an inch in thickness, I took from it forty very thin shavings weighing 19·9 grammes, but when thoroughly dried in the stove, at a temperature of 262° F. they weighed only 12·45 grammes.

From this experiment, it is evident that the wood in question, being full of sap, was composed of 12·45 ligneous parts, and 7·45 parts of water, or of sap, whose specific gravity is nearly the same as that of water.

Now, as one cubic inch of this wood weighed 19·134 grammes, it is very certain that it was composed of 11·971 grammes of ligneous parts, which were, consequently, solids, and of 7·168 grammes of sap.

But we have already seen, from the results of the experiments detailed in the former part of this memoir†, that a cubic inch of the solid parts of the wood of the oak, weighs 30·42 grammes; consequently, the 11·971 grammes of solid parts

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[*] In order to determine and keep an account of the quantity of water remaining on the surface of this piece of wood at the instant of withdrawing it from the vessel, it was weighed when taken out, whilst still quite wet. As its weight had been taken previously to the operation, the augmentation it had acquired from the water was ascertained to a nicety.

The vessel when empty weighed 188·22 grammes, and when filled with water at the temperature of 60 F. 474·9 grammes; so that it contained 286·68 grammes of water. When the piece of wood was plunged into the water, a small glass plate, about two inches in diameter, and two lines in thickness, ground with emery, to fit it to the edges of the vessel, so as to close it hermetically, was laid upon its mouth to shut up the piece of wood with the water still remaining in the vessel, whilst its outside was wiped with a dry cloth.

When the exterior of the vessel had been thoroughly dried, the glass cover was carefully removed, and the piece of wood withdrawn; the vessel was then weighed again with its remaining contents of water; and from its weight the quantity of water displaced by the wood, was calculated.

[†] See the table, page 323.

found

Sap, &c. in different kinds of wood.

found in one cubic inch of this wood, when the tree was alive, could have no greater bulk than..........	0·39353 cubic inch.
As one cubic inch of water weighs 19·83 grammes, the 7·163 grammes of sap, found in the cubic inch of this wood, must have occupied a bulk equal to....	0·36122
Consequently, a cubic inch of the wood in question, contained a quantity of air, whose bulk was equal to..............	0·24525
Making together.............	1·00000 cubic inch.

We conclude from these results, that a young oak, in a growing state, at the beginning of September, when the wood appears to be diffused with sap, contains, nevertheless, about a fourth of its bulk of air, and that its solid ligneous parts do not make quite 4-10ths of its bulk. But we shall presently see that the lighter woods contain still less of ligneous parts, and more of air, than the oak.

A young Italian poplar, three inches in diameter, measured at two feet above the earth, was cut down on the 6th of September, whilst the tree appeared to be in a growing state. The specific gravity of a piece taken from the middle of the trunk, was found to be 57·946; consequently, a cubic inch of this wood weighed 11·49 grammes.

From a piece of this wood, apparently full of sap, 40 thin shavings were taken, six inches in length, and half an inch broad. The wood from which these shavings were planed, weighed 12·37 grammes, and the shavings, when thoroughly dried in the stove, weighed 7·5 grammes*.

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* As the heat excited by the plane in taking off these shavings, was sufficient to evaporate a very sensible quantity of sap belonging to the wood from which they were cut, the shavings became perceptibly dry during the operation; for I found that 40 thin shavings sometimes lost more than one gramme (about 1-12th of their weight) in less than a minute. In order to obtain their true weight, whilst they still remained part of the wood, I adopted the precaution of weighing the piece of wood, both the moment before, and the moment after the operation of planing. The difference in the weight of the wood, under these two circumstances, indicates the weight necessary to be given to the shavings, and which is here always attributed to them.

We

Sap, &c. in different kinds of wood.

We hence conclude, that a cubic inch of this wood, in its original state, while the tree was still alive, contained 7·1531 grammes of ligneous parts, which formed the fabric of the wood, and 4·3369 grammes of sap, differing in its specific gravity, little or nothing from common water.

As one cubic inch of the solid parts of this kind of wood weighs 29·45 grammes*, the 7·1531 grammes of ligneous parts found in a cubic inch of the trunk of the living tree, in September, could only have occupied the space of

	0·24289 cubic inch.
And the 4·3369 grammes of sap, contained in it, only..........................	0·21880
Consequently, in one cubic inch of this wood, there was a bulk of air equal to..	0·53831
Total..............	1·00000 cubic inch.

The difference between the structure of the oak and of the poplar, becomes very conspicuous on making a comparison, according to the subjoined method, between the constituent parts of these two kinds of wood, both in a growing state.

Thus, a cubic inch of wood is composed of,

	Ligneous parts.	Sap.	Air.
The oak.......	0·39353	0·36122	0·24525
The poplar......	0·24289	0·21880	0·53831

This striking difference, in the proportions of the ligneous substance of sap and of air, discovered in these two species, sufficiently explain the difference observable in their weight and hardness. This inquiry may probably lead to other discoveries of more general utility in the study of the vegetable economy.

SECTION III.

Of the relative quantities of Sap and Air found in the same Tree, in Winter and in Summer; and in different portions of the same Tree, at the same time.

The following experiments were undertaken with a view to

discover

---

* See the table, page 323.

Sap, &c. in wood at different seasons.

discover the difference between the quantities of sap and air found in the wood composing the trunk of a large tree, in winter and in summer.

On the 20th of January, 1812, I had a lime-tree felled, of about 25 or 30 years growth, which had stood among several others of the same age in my garden at Anteuil. On taking a piece of wood from the middle of the trunk, at about three feet above the ground, it appeared to be filled, and even drowned in sap. Its specific gravity was 76617; consequently, one cubic inch of the wood weighed 15·788 grammes.

Having planed off 10 grammes of thin shavings from this piece, and dried them thoroughly in the stove, I found their weight reduced to 4·72 grammes.

Thus in possession of the specific gravity of the solid part of this wood, it was easy to determine, with the aid of these data, the constituent parts of a cubic inch, which were as follow:

Ligneous parts	0·25353 cubic inch.
Sap	0·44549
Air	0·30098
	1·00000

On the 8th of September, in the same year, (1812) I had a piece of wood (=5·84 cubic inches) cut from the trunk of another lime, of equal age with the former, (from 25 to 30 years) at the height of three feet above the earth. This tree was in a growing state, and the piece taken from it, after it had been trimmed by the joiner, weighed 87·8 grammes, and displaced 115·8 grammes of water, at the temperature of 62° F. consequently, its specific gravity was 75820. In the month of January, the specific gravity of this same species of wood had been found to be 79617.

From the piece of wood taken from the tree on the 8th of September, I had 14·19 grammes of their shavings planed off, which, after they had been thoroughly dried in the stove, weighed only 7·35 grammes. Hence we have, as the constituent parts of a cubic inch of this wood—

Ligneous

Sap, &c. in wood.

Ligneous parts....................	0·26489	cubic inch.
Sap..............................	0·36546	
Air..............................	0·36965	
	1·00000	

From the results of these two experiments, we may conclude, that the body of a tree contains more sap in the winter than in summer, and more air in summer than in winter. But the following experiments demonstrate the sap to be very disproportionately distributed in the several parts of the same tree, at the same season.

On the 8th of September, I had a branch, about three inches in diameter, cut from the lime just spoken of, and which issued from the trunk at the height of ten feet above the surface of the earth. From the lower end of this branch, I took a piece of wood, and subjected it to the investigation requisite to ascertain its constituent parts.

Its specific gravity was 70201. The same day, I found the specific gravity of a piece of the trunk of the same tree, to be 75820.

Surprising as this difference appeared, my astonishment was still more excited, on finding that a piece of wood, of three years growth, cut from the upper end of the same branch, where it was but one inch in diameter, had a specific gravity of 85240.

There was, therefore, much more sap, and less air, in the wood of the upper extremity of the branch, than in the lower, which was nearer to the body of the tree.

I afterwards examined the young shoots of the current year, in the same tree, as well as in several other species of wood, and uniformly found that the specific gravity of the young wood, that is to say, of the current year, is always considerably greater than that of the same species of wood when grown older. Doubtlessly, because it contains more sap, and less air, than the old wood.

In the management of experiments for determining the specific gravity of wood of the current year, it is indispensably necessary to take an account of the space occupied by the pith, without which precaution, we shall be led to false conclusions.

Sap and volatile parts in wood.

I found the specific gravity of the oak of the current year to be 116530; that of the elm, 110540. Young shoots of these trees, deprived of their bark and pith, descend rapidly on being thrown into water; whilst pieces of the same tree, more advanced in age, swim on the surface, even when the wood is green, and more full of sap.

This fact is worthy the attention of persons occupied in the study of vegetable physiology.

I was next curious to examine the root of the lime from which I had already had one piece of wood from the trunk, and two pieces from one of its branches. With this view, on the 8th of September, 1812, I caused one of its roots, of about two inches diameter, to be taken up, and cut from it a piece weighing 93·25 grammes, which displaced 115·8 grammes of water. Its specific gravity was 80527, and, consequently, greater than that of the wood extracted from the trunk of the same tree, but less than that cut from the upper end of one of its branches. 20·48 grammes of thin shavings, from this piece of the root of the lime, weighed only 10·85 grammes after being thoroughly dried in the stove.

From these data, I determined the constituent parts of a cubic inch of the root, thus:

Ligneous parts.	0·28775 cubic inch.
Sap.	0·37358
Air.	0·33867
	1·00000

The constituent parts of a cubic inch of the body of the same tree, were, as we have shewn:

Ligneous parts.	0·26489 cubic inch.
Sap.	0.36546
Air.	0·36965
	1·00000

The constituent parts of a cubic inch of the wood of the same tree, taken the same day from the lower extremity of a branch, were:

Ligneous

Sap and volatile parts in wood.

Ligneous parts	0·25713 cubic inch.
Sap	0·27513
Air	0·46774
	1·00000

Lastly, the constituent parts of a cubic inch of the wood, taken near the upper extremity of the same branch, were:

Ligneous parts	0·25388 cubic inch.
Sap	0·47599
Air	0·27013
	1·00000

For the more easy comparison of the results of these four experiments upon the wood of the lime tree, made on the same day, with different portions of the same tree, I have collected them together in the following

TABLE.

	A cubic inch of wood was composed of		
	Ligneous parts.	Sap.	Air.
The root	0·28775	0·37358	0·33867
The trunk	0·26489	0·36546	0·36956
The lower end of a branch	0·25713	0·27513	0·46774
The upper end of ditto	0·25388	0·47599	0·27013
Wood taken from the trunk of a lime tree of the same age, on the 20th of Jan.	0·25353	0·44549	0·30098

Being desirous to ascertain whether a difference considerable enough to be valued, existed between the wood of the heart, or core, and the sap wood found between the rhind and the body of the same tree; I took on the 11th of September, an elm faggot, five inches in diameter, lopped from a large tree, which had been felled on the 20th of the preceding April, and had two cylindrical pieces, each six inches in length, cut out of it. The thickest of these taken from the core, weighed 191·05 grammes, and displaced 194·45 grammes of water; the other,

consisting

Sap and volatile parts in wood.

consisting of the sap-wood, weighed 93·61 grammes, and displaced 111·45 grammes of water.

The specific gravity of the core was, therefore, 98251; that of the sap-wood, 81764. But as the faggot had lain exposed to all the summer rains, the wood was far from being dry. I was, however, much surprised at discovering, that the core of this wood was more charged with sap, or water, than that of the same kind of wood when in a growing state. A fact which induces a suspicion, that the sap in trees is not enclosed in vessels or tubes apparently impervious to that liquid.

To obtain a better knowledge of the wood in question, I planed off 40 shavings, six inches in length, and half an inch in breadth, from a small board cut from the core; with an equal number of shavings, of similar dimensions, from another board cut from the sap-wood.

The 40 shavings from the core, taken just as they were planed off, weighed 16·37 grammes, and 10·53 grammes after they had been thoroughly dried in the stove.

The 40 shavings of sap-wood weighed 16·97 grammes before they were dried, and 11·99 grammes afterwards.

Thus possessed of the specific gravity of the solid parts of this kind of wood, it only remained to determine, from these data, the constituent parts of an inch of the wood, which was readily performed, as follows:

	Ligneous parts.	Sap.	Air.
In the core of the elm. .	0·41622	0·35055	0·23223
In the sap-wood........	0·38934	0·23994	0·37072

It appears, from the results of these experiments, that the sap-wood of the elm contains rather more ligneous parts in its timber, than the core of the same tree; and that it contains much less sap, and more air. But as the tree had been felled nearly five months before it became the subject of investigation, it is very possible that the sap wood had become much drier than the core of the tree.

I had purposed to repeat these experiments upon wood in a growing state, and upon seer-wood; but the interference of other occupations has prevented a continuance of the inquiry. It cannot, however, but lead to results curious in themselves; and I therefore recommend it to the notice of all students in vegetable economy, as well as to those who love that noble science, and feel a gratification in being able to remove the veil under which the mysterious operations of nature are concealed.

Sap and volatile parts in wood.

The particular object which I had in view in exploring the structure of wood, have led me by a way by no means likely to be fertile in interesting discoveries: but I have begun the work, and feel myself bound to complete it, in preference to every other consideration. These fascinating researches, I am aware, have already carried me too far, and I must now resign them into the hands of others, in order to fulfil my engagements. This I do most cheerfully, and it will give me the greatest pleasure to behold a field too long neglected once more broken up.

Section IV.

Of the Quantities of Water contained in Woods considered as dry, or Seer-Woods.

Wood is an hygrometric substance, and when exposed to the atmospheric air, always imbibes a visible quantity of water; varying, however, with the temperature and humidity of the air.

If the moisture in the wood were confined in vessels so constructed as to be totally impervious to water, the fabric of the wood would be uniformly the same, with the exception only of the variations caused in its dimensions by change of temperature; in which case, it would be very easy to determine the quantity of water contained in the wood, when the specific gravity of its solid parts was known. But as the bulk of all woods is considerably diminished in drying, the experiment is rendered rather prolix, though by no means difficult, and its results are clear and satisfactory.

A few examples will suffice to point out the method to be pursued.

The composition of the oak, in a growing state, at the beginning of September has been already given. In order to ascertain the change which this wood undergoes by the process of drying, I made the following experiment.

From a faggot of oak, 5½ inches in diameter, which, covered with its bark, had been exposed to dry in the open air, for 18 months, I took a piece of rather more than an inch square, and six inches in length; it was good fire-wood, and seemed very dry.

This

Sap and volatile parts in dry wood.

This piece, after being trimmed by the joiner, weighed 126·2 grammes, and displaced 157·05 grammes of water; its specific gravity was consequently 80357, and a cubic inch weighed 15·939 grammes.

Forty-three shavings of this wood, six inches long, and half an inch broad, weighed 17·9 grammes; but when thoroughly dried in the stove, they were reduced to 13·7 grammes. They were, therefore, prior to being put into the stove, composed of 13·7 grammes of solid parts, that is to say, of dry, or seer-wood, and 4·2 grammes of water.

The results of this experiment indicate, that 100 kilogrammes of this excellent fire-wood contained 76 kilogrammes of seer-wood, and 24 of water; which is, probably, the ordinary state of the best fire-wood sold in the timber-yards of Paris, and all other places.

Were the wood to be kept for several years, in a dry place, secured from the rain, it is possible, that it might become dry to such a degree as to contain only about 12 per cent. of water, and 88 of seer-wood! But it will appear in the sequel, that wood of any kind, exposed to the atmosphere, could never become more dry, on account of its hygrometric quality, which it constantly preserves.

The following are the constituent parts of a cubic inch of fire-wood employed in this experiment:

Ligneous parts, or seer-wood......	0·40166 cubic inch
Sap, or water..................	0·18982
Air........................	0·40852
	1·00000

Thus we are enabled clearly to demonstrate the difference between the oak in a growing state, and the same kind of wood after it has been felled and dried in the air, secured from the rain, for 18 months.

	Dry wood	Water.	Air.
In a cubic inch of oak, in a growing state.........	0·39353	0·36122	0·24525
In a cubic inch of the same kind of wood, after it had been felled and dried for 18 months..........	0·40166	0·18982	0·40852

By comparing the relative quantities of sear-wood contained in a piece of timber while in a growing state, and in the same timber after it has been dried, we may ascertain how much its fabric has shrunk by dessication.

It appears from these experiments, that the oak sold in the timber-yards of Paris, for fire-wood, contains rather more than one-half of the sap which it formerly had, in a growing state.

I have made several similar experiments upon other species of wood; but their results are better calculated for exhibition in a table, than for circumstantial detail.

(To be continued.)

III.

Description of the perpendicular Lift erected as a Substitute for Locks on the Worcester and Birmingham Canal at Tardebig, near Bromsgrove. By Mr. Woodhouse. *From a printed Letter of Mr. Edward Smith, of Birmingham, and the Reports of W. Jessop, Esq.*

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

THE whole of the machinery is under cover; and we entered the building at the lower level of the canal, where the appearance of a number of large wheels, rods, and chains, seen in perspective, had a very striking and pleasing effect. We walked by the side of an oblong trough or vessel, filled with water, large enough for a canal boat to float in. This reservoir of water, with the canal boat, weighs sixty-four tons, and is suspended by eight rods and chains over as many large cast-iron wheels or pullies, which are balanced on the other side of the wall by an equal number of square frames, loaded with brick-work, or other heavy materials. After examining the lower structure of the building and machine, we got into an empty boat which floated in the reservoir, and were slowly raised to the upper level of the canal, without any noise or jarring of the machinery, by means of wheels and pinions on the other side, which were worked by two men with great ease; it took about three minutes to ascend twelve feet, the difference between the two levels. When the trough is thus raised to the necessary height, the paddles at the end, which are ingeniously con-

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

from my house at Bordesley, a large fire engine, which till of late was an insufferable nuisance to the neighbourhood, by the immense volumes of thick black smoke it was pouring out, night and day, without intermission. This inconvenience of the smoke has been lessened, in a great degree, by a contrivance in the management of the fire place, which ought to be adopted in all such cases. When I observe this engine employed solely in throwing back water to the upper level of the Warwick canal, for the floating of the boats, up and down through half a dozen locks, within the space of half a mile, I cannot help considering, that had the lift been known and applied, the canal might, at a little expence, have been continued on a level to the place where the fire engine is constructed, the expence of working the engine and all the lockage saved, and the boat, by one lift removed from one level to the other; the first cost of the lift, no doubt, would be great, and then you have said nearly the whole; no fear of dry seasons, the reservoirs and feeders being sufficient to supply the loss of water from exhalation by the summer sun.

Again, to look at the Birmingham canal at Smethwick, with its fire engine, reservoirs, and double range of locks---to what advantage might this machine be applied in such a situation!

Plate VIII. Is a perspective view of the internal part of the machine, when viewed from the lower level of the canal. The surrounding walls are taken away to avoid confusion: the centre wall is also broken off, at the nearest end, in order to shew the manner in which the balancing weights, at the back, are suspended. The better to display the construction of the trough, it is raised five feet above the lower level; the dimensions of it are as follows; length 72 feet---breadth 8 feet---depth 4 feet 6 inches, all outside measures. It is composed of planks, 3 inches thick; its weight, when filled to the proper height with water, is 64 tons. The paddles, and their appurtenances, are marked as clearly as the nature of the case would admit; a further explanation of them, will be found in the references to figures 2, 3 and 4, Plate IX.

From the corners of the trough, rise four strong posts, 12 feet high, in each of which is a groove, which receives the respective paddles. Parallel to these, are similar posts, in which slide the paddles of the canal. When a boat is to be introduced at the

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

lower level, the narrow space between the paddle of the trough and that of the canal, is first filled with water, by opening a valve, the situation of which is pointed out by the letter *H*, in fig. 2 and 4, Plate IX. ; the lateral pressure of the water against the paddles, is thus removed : The small chain, which hangs down between the upright posts, the lower part of which is double, is then linked to the hooks of both paddles, and by means of the crane near the end, they are drawn up together, and the boat floats into the trough ; the paddles are then dropped, and the trough raised to the upper level, when the boat is liberated by opening the paddles at the contrary end. A similar operation takes place, when a boat is required to descend from the upper to the lower level.

Plate IX. Figure 1, a section of the end of the machine ; this clearly shews the principle by which the weight is raised, viz. that of the simple pulley, where the weight suspended on each side being equal, a force sufficient to overcome the unavoidable friction being applied, puts the whole in motion either way. *A A* represents the section of the trough, suspended from the iron beam *C D C*, by rods, the lower ends of which are fastened by screws and nuts at *B B*, and the upper ends are fixed in the same manner at *C C*.

From the centre *D* of the beam *C D C*, proceeds a very strong double chain *D D d E*, passing over the wheel *H H*. From the end *E* hangs an iron rod *E F G*, which passes through a thick square platform of oak at *G*, loaded with brick-work to the weight of eight tons ; this is the case with each of the others ; the weight, therefore, of the whole, is 64 tons, being equal to that of the trough, which they hold in equipoise.

H H, a cast-iron wheel, 12 feet in diameter, one of the eight which are seen in Plate I. *I K*, the centre wall, 30 feet high.

From *L*, under the centre of the trough, is suspended a chain, which is loaded at equal distances with blocks of iron, 1, 2, 3, 4, 5 ; the weight of them is equal to as much of the chain and rod *D d E F G*, as hang in a perpendicular direction. The weights *F G*, are provided with similar chains, so that, as in the present instance, when they are at the lower level, the opposite blocks are called into action, and counterbalance the force of that portion of chain and rod, extending from *d* to *G*, while the

blocks

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

blocks suspended from *G* lie inactive in the cavity *M N*: the contrary is the case when the trough is sunk.

Figure 2, is a section of the paddles, &c. of the upper level. *Aa, Bb*, the two perpendicular posts, containing the grooves, in which the paddles *C D* slide. *E F*, small wheels at the extremities of the posts; these, by rolling against other surfaces, contribute to regulate the ascending and descending motion of the trough. This is more distinctly seen in Figure 3, which is a profile of this part.

G G, is the bottom of the canal, which projects a little beyond its paddle, in order to fill up the space between the bottoms of the two paddles, through which the water would otherwise escape. The side spaces are filled up by square pieces of wood, slide against strips of thick felt; thus rendering the whole completely water-tight. *H*, the small valve, by withdrawing which the space between the paddles is filled with water.

Figure 4. A plan of the situations of the paddles in the grooves; which is sufficiently explained by comparative reference to the other plates.

Plate X. gives an elevation of the back of the machine, shewing the eight wheels *H* 1, *H* 2, &c. the chains and rods *D E F*, and the poise weights *F G*. Here also the external building is removed, the centre wall alone remaining, in the interstices of which the wheels revolve.

The wheels No. 2 and 7, are toothed through twelve feet of their circumference, and by means of these teeth, they are acted upon by the wheel-work, which this plate also exhibits. *I I* are the two winches by which the pinions and wheels *K K, L L, M M, N N, O O*, are turned, and sufficient power is thus acquired to move the wheels *H H*; this is effected by the teeth of the pinions *O O*, meeting those of *H* 2 and *H* 7; these two being connected by the common axis *P P*, their motions necessarily correspond. On the sides of the weights *F G*, are small projections, which slide into grooves, constructed in the upright posts *Q R*. By means of these grooves, and the regulating wheels *E F*, in figures 2 and 3, Plate II. the perpendicular motion is rendered so perfectly true, that it was judged unnecessary to give the wheels *H H*, any hollow; the chains consequently move on flat surfaces, depending only on the mathematical truth of the work.

The

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

The lines *S*, *T*, point out the situations of the lower and upper levels of the canal, between which, as was before observed, the fall is 12 feet.

W. H. SMITH.

Since my letter was first written a number of improvements have been made in the perpendicular lift by the ingenious projector, to prevent the chance of accidents to which new schemes are exposed, and also to obviate several objections that have been industriously circulated against the machine. An apparatus has been added, which effectually prevents the sudden motion of the machine, in case any accident should happen while the conductor is at the lower level, and which renders it impossible for the weights to descend, till the paddles of the conductor are adjusted.

Two pumps have also been introduced, which regulate the speed of the machine, and by which the conductor may pass from one level to the other, without any manual labour; and the conductor and paddles are now so guarded, that they cannot receive injury from the violent entrance of the boats.

Owing to the numerous delays, the tunnel at Tardebig was not completed so soon as stated, and the consequent trial of the lift, as expected by the proprietors, could not be effectually made previous to the general meeting of January 1, 1811. This, and other circumstances, induced the general meeting to pass resolutions, by which it was determined, (though so much expence had been incurred) to abandon the scheme *in toto*, and finish the canal by means of locks; chiefly, however, on the ground, that it was impossible (as alleged) for the lift to pass nearly the number of boats requisite, when the canal should be completed. Several respectable proprietors, not satisfied with this determination, and concerned that a plan, in their opinion, replete with advantage to the canal and the public at large, should be abandoned almost without trial, have come forward at their own expence, to make a complete trial of the machine, and its capability to do what might be requisite.

This trial was continued under the patronage of these gentlemen for nearly a month, by means of three boats constantly working upward and downward, for a given period in each day, one of 20 tons, one of 15 tons, and the other empty, being the usual

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

usual proportion in the common traffic on canals. The result of this will surprize many persons who had formed a very different idea of the power of the lift, and must be very gratifying to the inventor, who, whether the scheme be adopted or abandoned by the proprietors, will be able to refer to the solid test of experiments thus laid before the public. The result of the experiments to the day of publication I subjoin

E. SMITH.

March 18, 1811.

Feb. 25,	1st Day,	50 Boats passed,		in 6 Hours 29 Min.
—— 26,	2nd	60	ditto	in 8 Hours 10 Min.
—— 27,	3d	70	ditto	in 9 Hours 8 Min.
—— 28,	4th	37	ditto	in 5 Hours 1 Min.
Mar. 1,	5th	50	ditto	in 6 Hours 40 Min.
—— 2,	6th	50	ditto	in 6 Hours 48 Min.
—— 4,	7th	50	ditto	in 6 Hours 52 Min.
—— 5,	8th	40	ditto	in 5 Hours 16 Min.
—— 6,	9th	51	ditto	in 6 Hours 21 Min.
—— 7,	10th	48	ditto	in 6 Hours 22 Min.
—— 9,	11th	41	ditto	in 5 Hours 20 Min.
—— 11,	12th	40	ditto	in 5 Hours 25 Min.
—— 12,	13th	100	ditto	in 11 Hours 46 Min.
—— 13,	14th	58	ditto	in 7 Hours 20 Min.
—— 14,	15th	50	ditto	in 5 Hours 41 Min.
—— 15,	16th	110	ditto	in 12 Hours.
—— 16,	17th	50	ditto	in 5 Hours 37 Min.
—— 18,	18th	50	ditto	in 5 Hours 54 Min.
—— 19,	19th	113	ditto	in 12 Hours.
—— 20,	20th	50	ditto	in 5 Hours 42 Min.
—— 21,	21st	68	ditto	in 8 Hours.
—— 22,	22d	60	ditto	in 6 Hours 26 Min.
—— 23,	23d	62	ditto	in 6 Hours.

(Signed) WILLIAM JOHNSON.

IV.

Curious Fact of the Outlines of Trees, accurately sketched on the surface of the ice on the Bog Lakes of Ireland. In a Letter from JOHN CHICHESTER, *M. D. of Bath.*

To W. Nicholson, Esq.

SIR,

Trees buried under the Bog Lakes are marked by the hoar frost being less perceptible over them.

THE account given in your Journal for April last, of the remarkable appearance of the ice in a pond in which a man lay drowned, brought to my recollection the following analogous, and perhaps no less curious phenomenon, occurring in the Bog Lakes of Ireland, communicated to me some years since by the Rev. Mr. Mangin. The following are Mr. Mangin's own words:—

"On the 24th of December, 1809, I was in company with a gentleman from Ireland, who mentioned what appeared singular, and was then new to me: speaking of the bogs in his neighbourhood, and of the large trees so frequently found in them, he said, that at those periods of the year, when the hoar frost fixes on the surfaces of the small lakes with which those morasses abound, he had repeatedly observed the form of a tree, (lying, perhaps, at a depth of fourteen or twenty feet beneath,) sketched most accurately on the ice above; that is to say, its length, breadth, and ramifications denoted by the frost not settling with equal force on those portions of the fluid under which the tree was extended, while the congealment was every where else more dense and complete." The gentleman added, that it was well known to the country people, who were accustomed to search for and find timber when thus indicated. The trees discovered in these places are of various kinds, oaks, elms, &c. and very commonly yew trees of vast size, their position invariably horizontal."

Without any comment, I beg leave to subscribe myself,

Sir,
Your obedient Servant,
JOHN CHICHESTER, M. D.
And Physician at Bath.

May 13*th*, 1812.

Annotation.

Though the indistinct outline of a large object not deeply immersed in a small stagnant pool, may seem to be well explained by the observations given at page 304, of our XXXIVth vol. Yet the same principles do not appear adequate to shew why the ramifications of a tree buried twenty feet in a bog, should be neatly figured upon the ice of water lying on its surface. None of the general operative powers with which we are acquainted, present a solution of this effect. Heat, electricity, gravitation. Of these the latter only is known to act in the perpendicular; but this affords no ground for the remotest conjecture. It would be desirable to know whether the ice of the outline were different in texture from the rest.—W. N.

V.

On Copper Wire, guilt with Brass. In a Letter from a Correspondent.

To Mr. Nicholson.

SIR,

Guilding by meansofbrass. I HAVE been informed, that the gilding of copper wire by means of brass is carried to great perfection in Germany. Perhaps it may also be the case in England; respecting which I should be glad to hear from your correspondents. The facts, as stated to me, are, that copper wire, coated with brass, is capable of being drawn out to the fineness of a hair; much finer than copper alone; that it is used for making gold lace, and the process effected in the humid way, as follows:—Take of zinc one part, and of mercury twelve parts—make a smooth soft amalgam, to which, if a little gold be added, it will be better. Clean the copper very nicely with nitric acid; put the amalgam into muriatic acid, and add argol or crude (not purified) tartar. Boil the clean copper in this, and it will be very finely gilt. Epaulets and gold-coloured trinkets are thus made very beautiful.

Query. Would this be an improvement in pins?

I am, Sir,
Your Constant Reader,
M. M. B.

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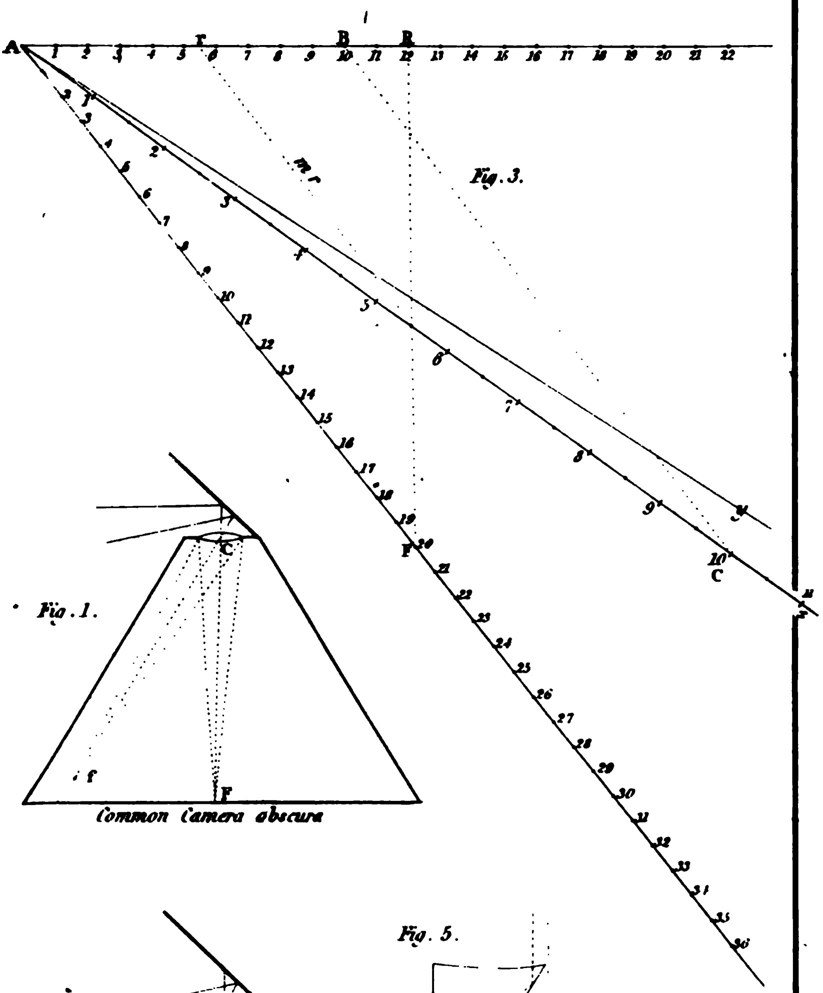

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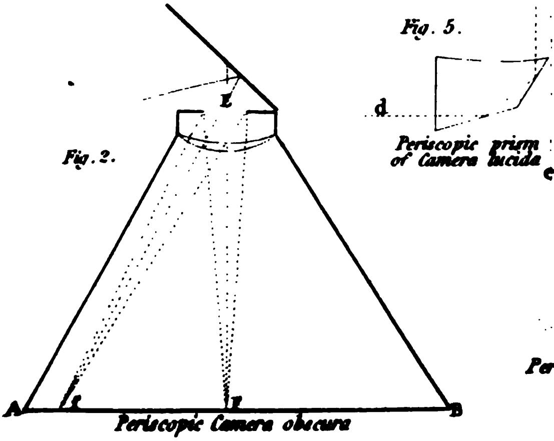

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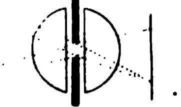

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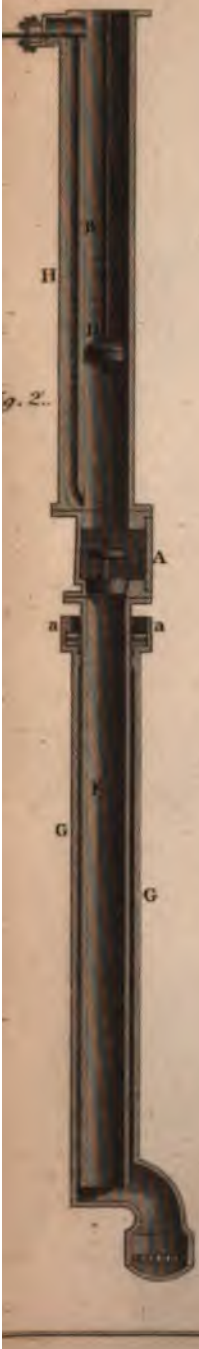

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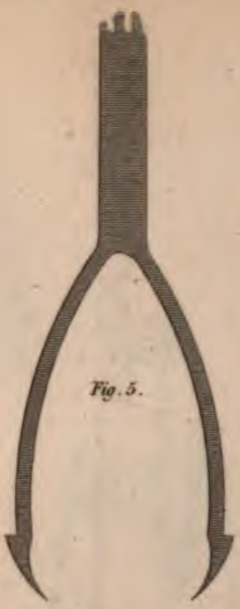

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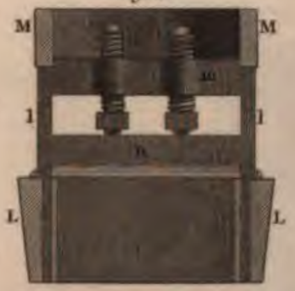

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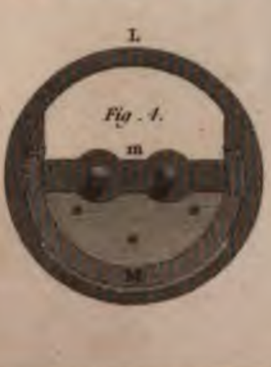

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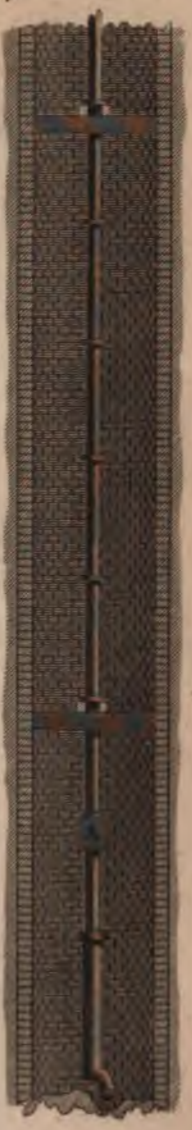

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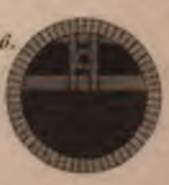

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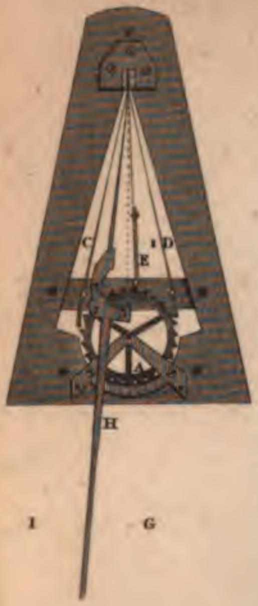

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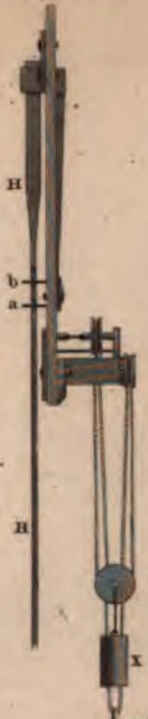

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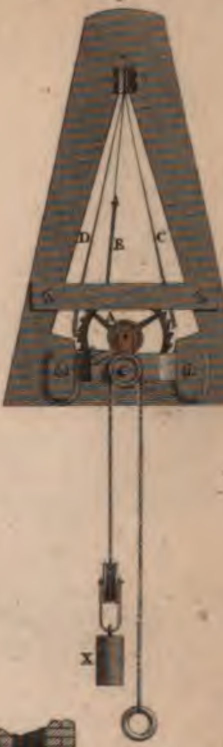

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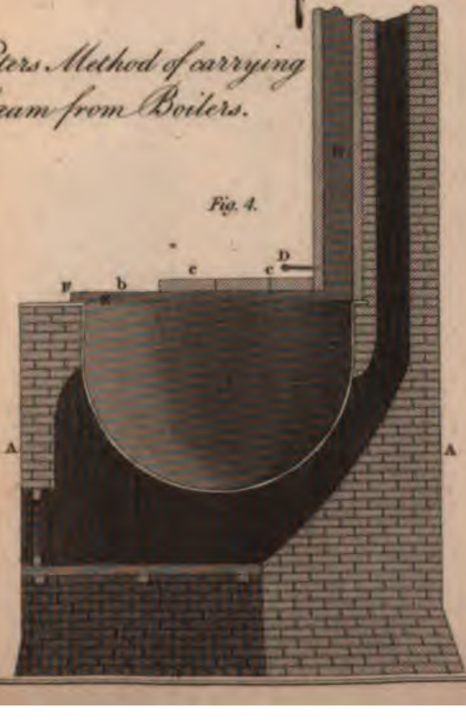

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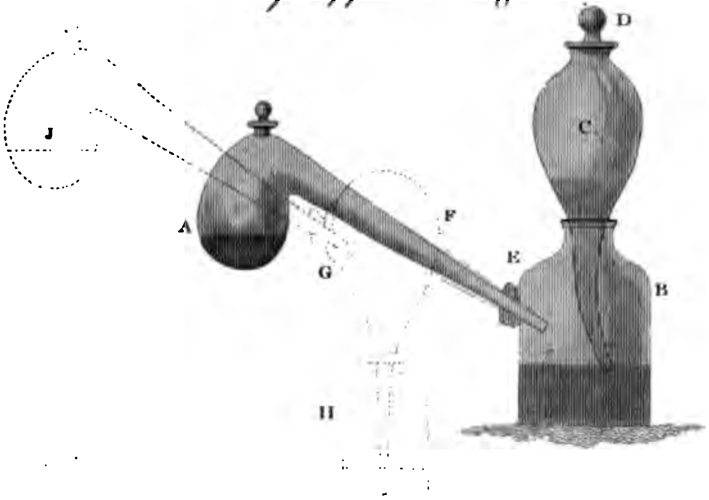

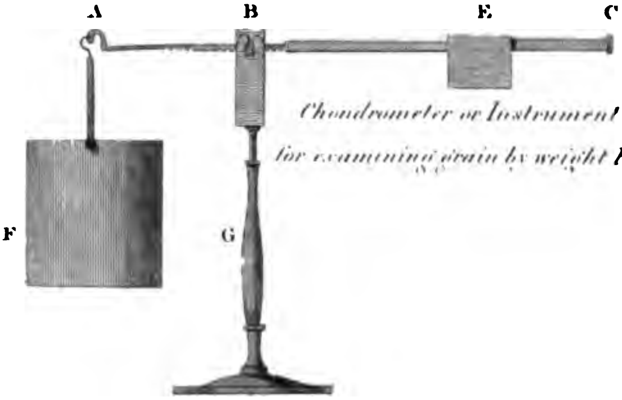

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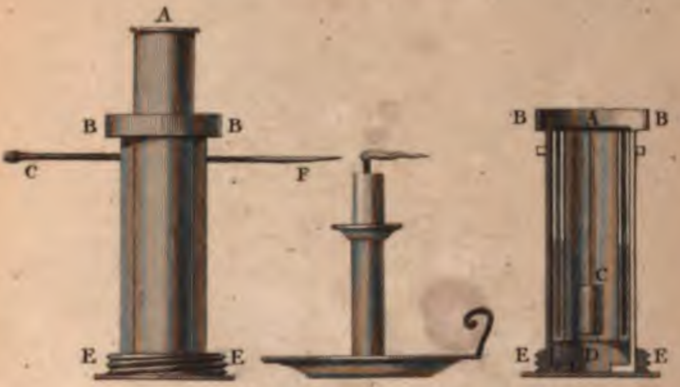

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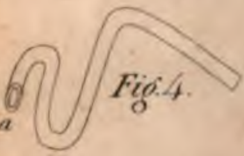

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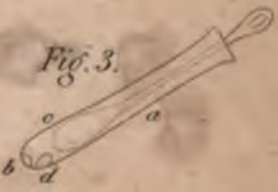

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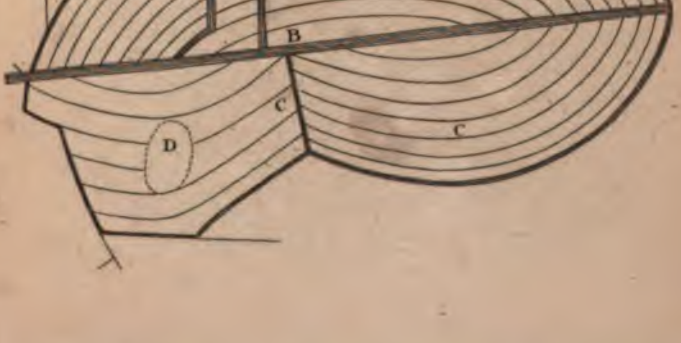

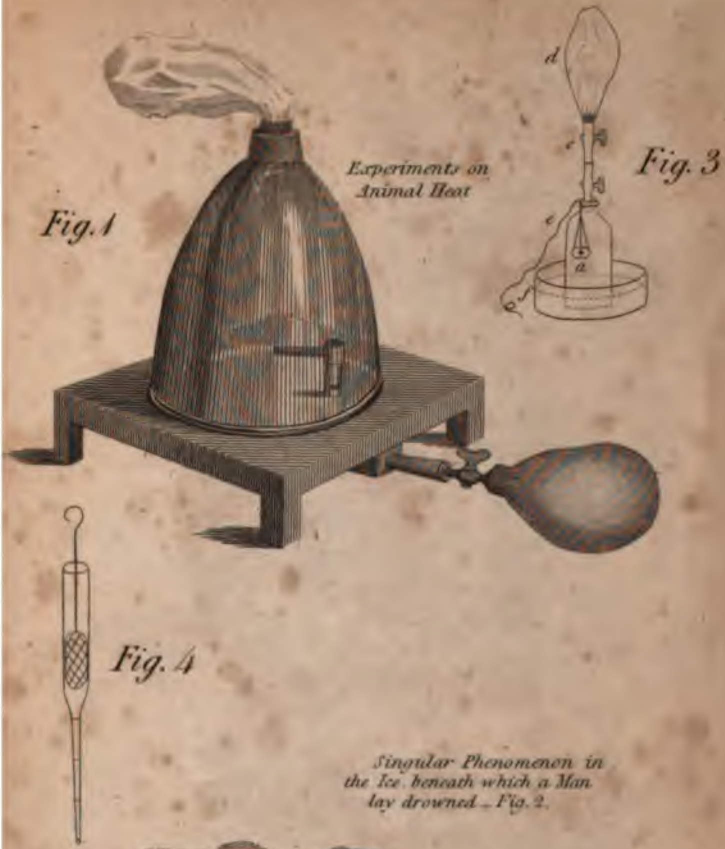

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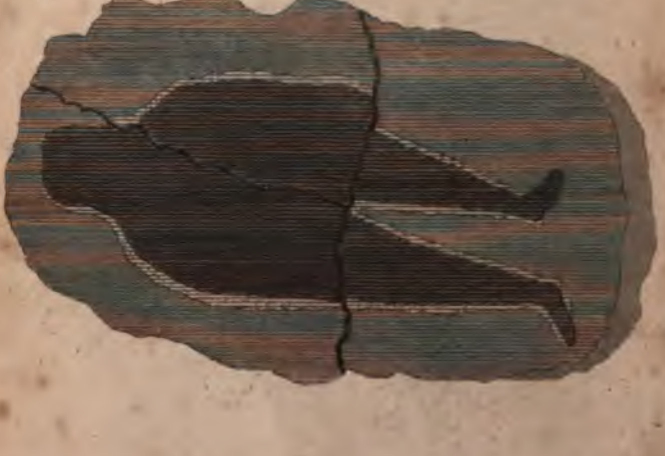

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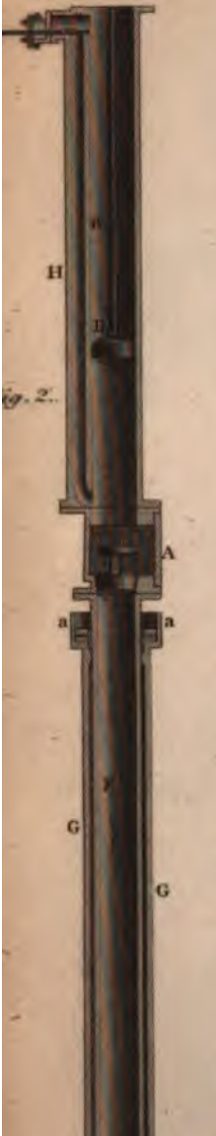
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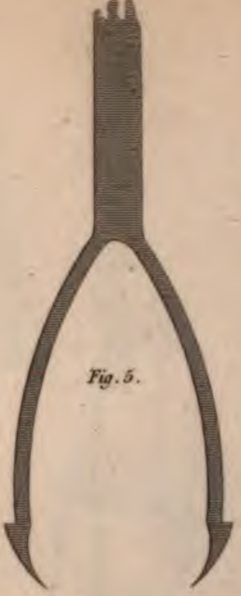
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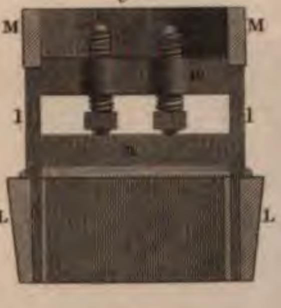
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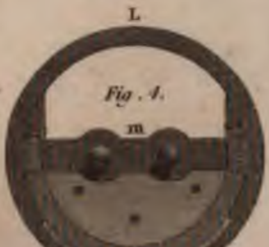
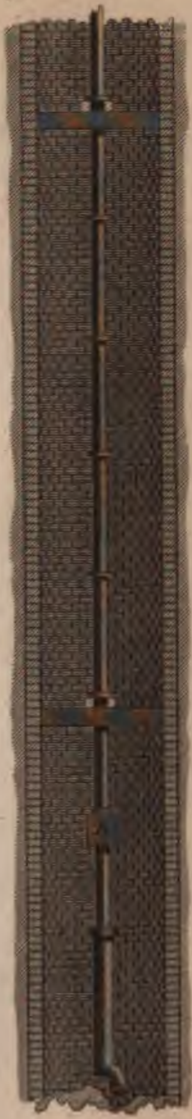


Fig. 4.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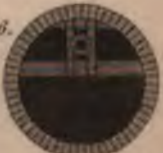
Fig. 6.

H. Cooper sculp.

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Remontoire Escapement by Mr. G. Prior Junr.

Fig. 1.

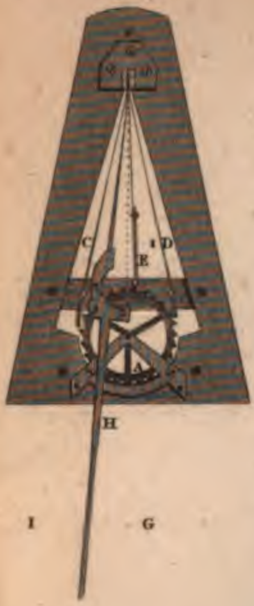


Fig. 2.

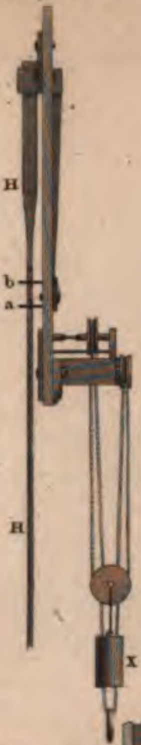
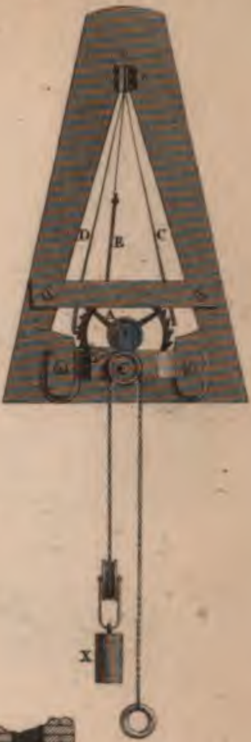
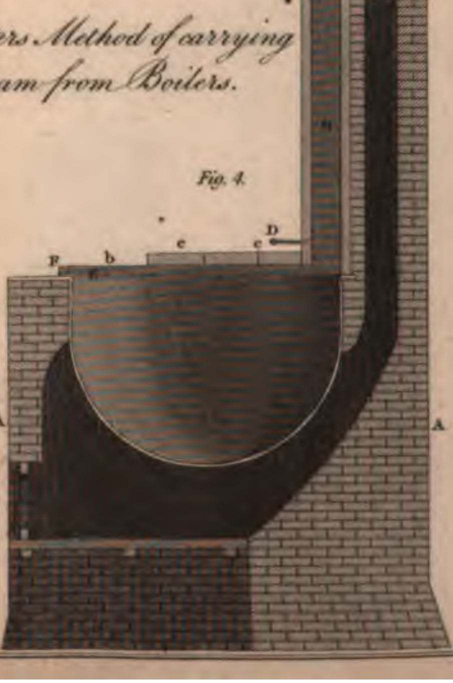


Fig. 3.



Mr. Websters Method of carrying off Steam from Boilers.

Fig. 4.



Cooper sc. Chancery Lane

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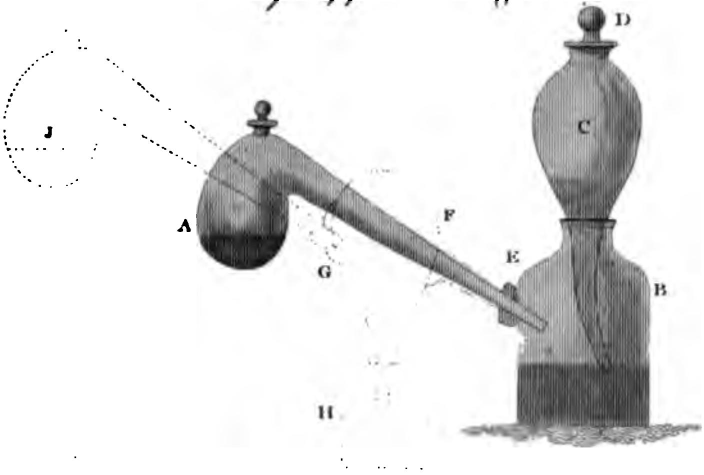
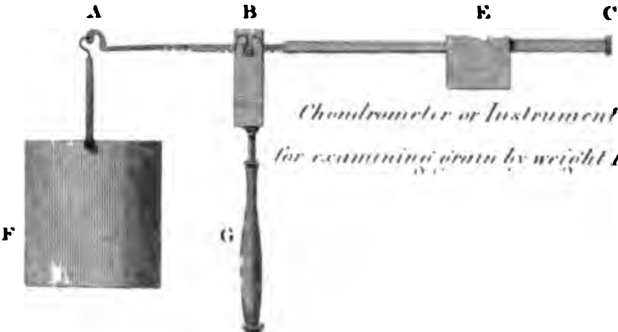
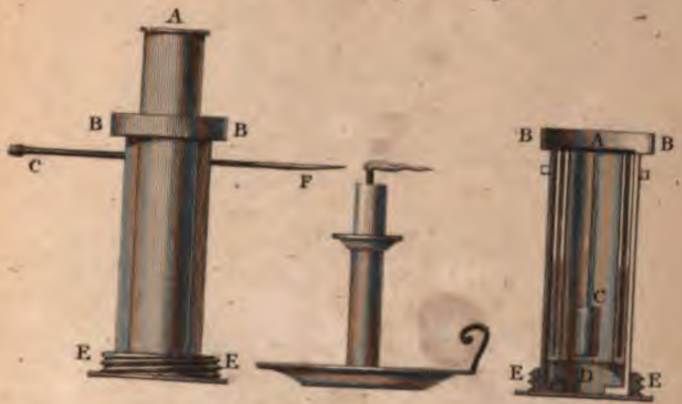
Distillatory Apparatus Fig. 1.





Chondrometer or Instrument for examining grain by weight Fig. 2

Statical Blow pipe Fig. 1.



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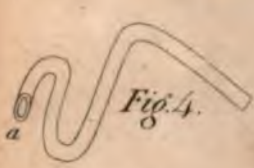


Fig. 3.

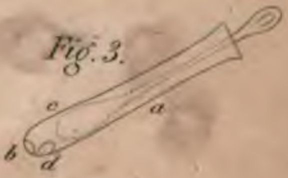
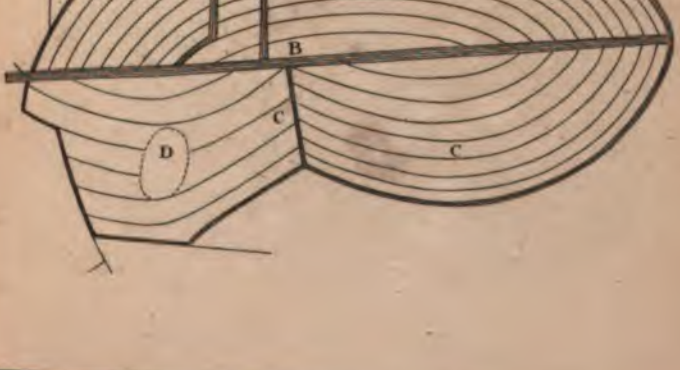


Fig. 2



Experiments on Animal Heat

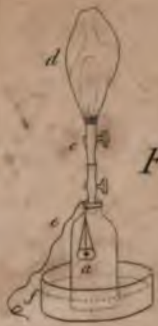
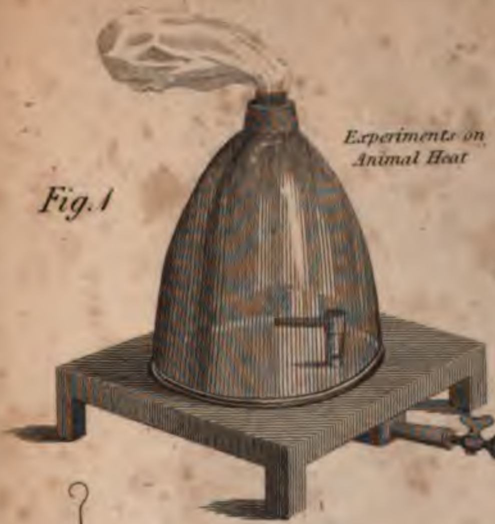


Fig. 3

Fig. 1



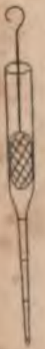
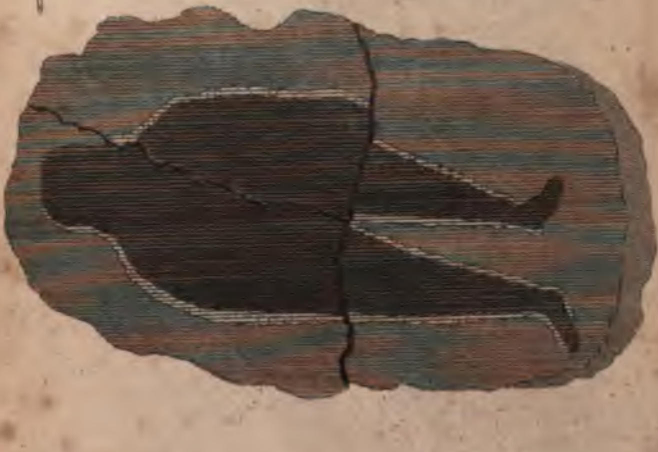


Fig. 4

Singular Phenomenon in the Ice. beneath which a Man lay drowned _ Fig. 2.



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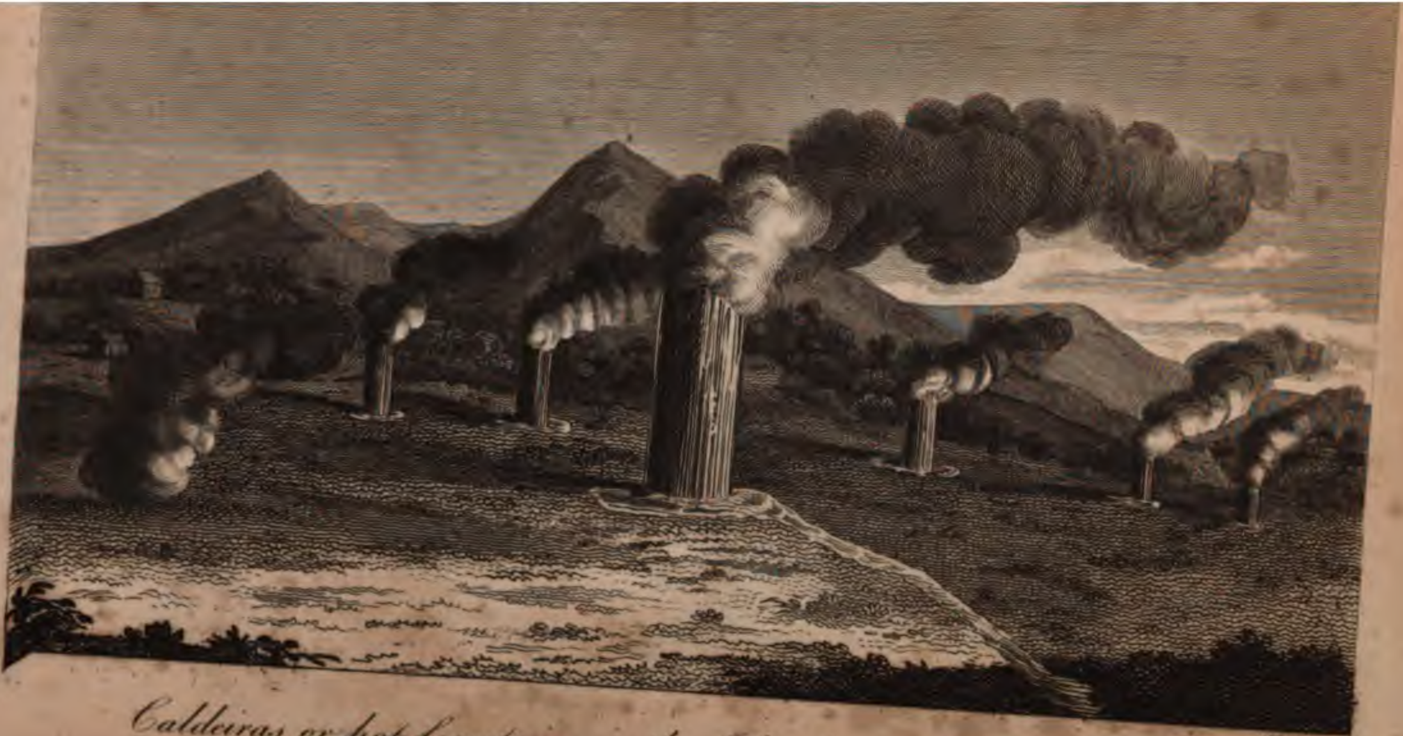

Cooper Sc.

Caldeiras or hot fountains in the Island of S.t Michael (Azores)

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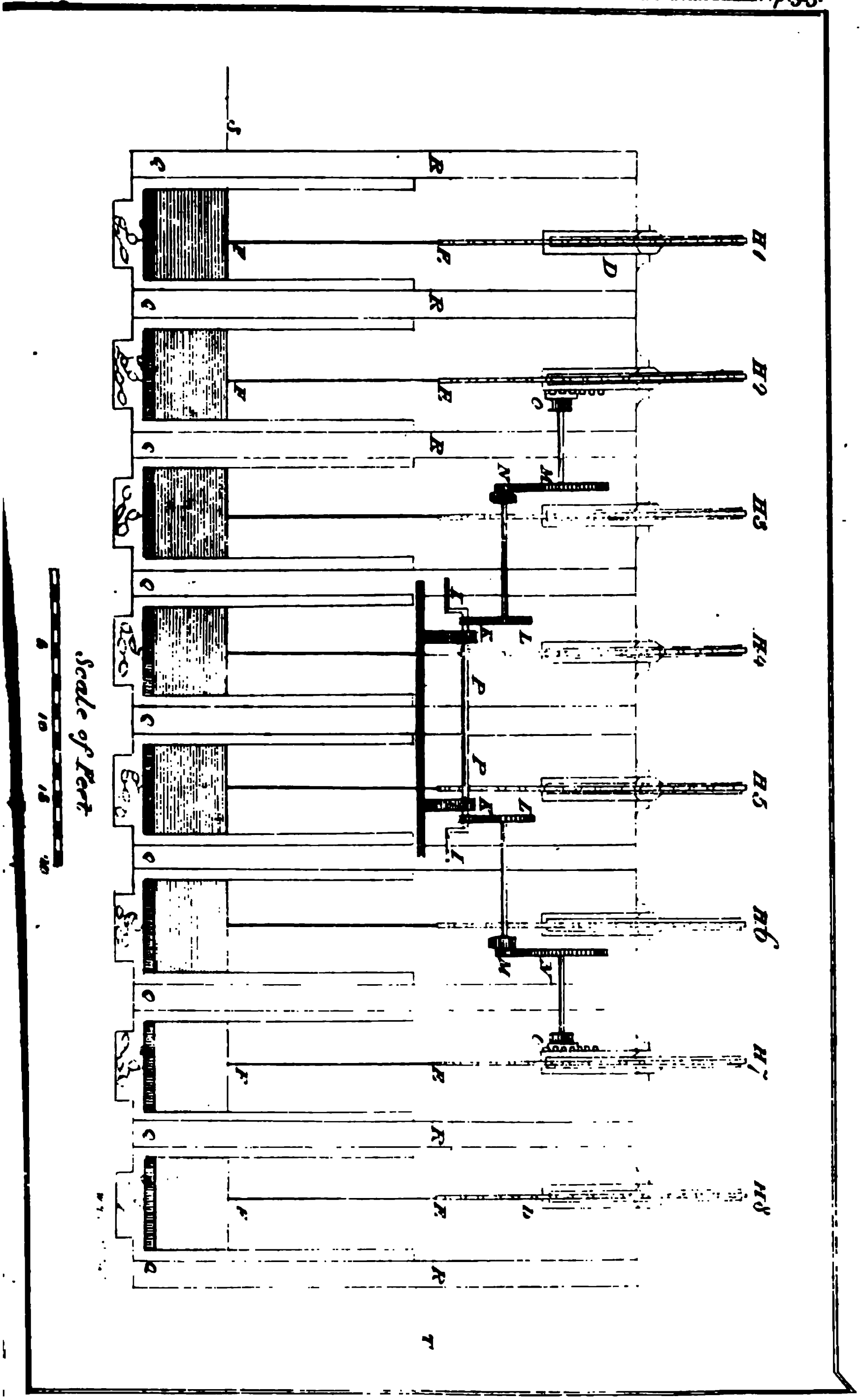
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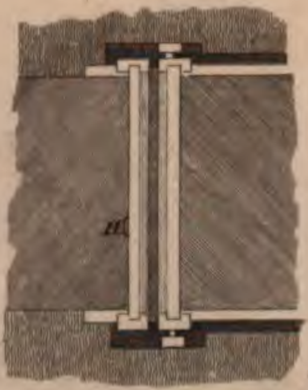
FIG. 4.



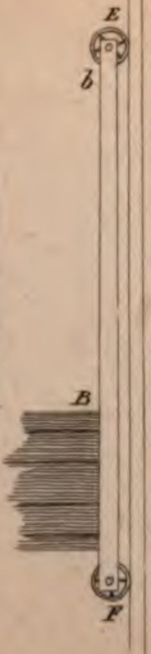


FIG. 2.